THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XXI.—*Coggia's Comet—its Physical Condition and Structure. Physical Theory of Comets;* by Prof. W. A. NORTON.

COGGIA'S Comet (Comet III, 1874), the last that was conspicuously visible to the naked eye, was attentively observed at observatories in both hemispheres. The reported results of the diverse series of observations and determinations made with the telescope, spectroscope, and polariscope, appear to reveal the essential features of its physical condition and structure, and to afford important indications with regard to the probable physical theory of comets in general. The following are the elements of the parabolic orbit of the comet, computed by M. Schulhof (Astr. Nachr., No. 2003). T=1874, July 8·85664, G. M. T., Π=271° 6′ 19″·5, Ω=118° 44′ 25″·3, i=66° 20′ 58″·6, log. q=9·829826, which gives, per. dist. q=0·67581. Motion direct.

Nature and Condition of the Cometic Matter.—The following are the general results of the observations made with the spectroscope and polariscope* with the view of ascertaining the nature and condition of the matter of the comet; in the nucleus and coma or envelopes, and as more widely diffused in the tail.

(1.) The light of the tail and coma was partially polarized in a plane through the axis of the tail.

(2.) The spectrum of the comet consisted of three or more bright bands on a continuous spectrum. This continuous spectrum was faint on July 7, but became much brighter by July 14. The three bright bands were identical with those obtained by passing a spark from an induction coil through gaseous-

* Month. Not. of Royal Astr. Soc., 1873–4, pp. 489 to 491; 1874–5, p. 83. Astr. Nachr., No. 2018, pp. 18 to 32.

dioxide of carbon (carbonic acid gas). Other experiments have shown that the volatile hydro-carbons give, with the electric spark, the same spectroscopic bands; and that these are wholly due to the momentary incandescence of the carbon molecules of the compound. Several other comets have given the same "carbon bands." Brorsen's comet, a faint circular nebula, invisible to the naked eye, proved to be an exception. Three bright bands were observed in its spectrum, but they differed in position and other features from the carbon bands.

(3.) "The spectrum of the nucleus was continuous, but it appeared to have traces of numerous bright bands, and three or four dark lines also were seen."

(4.) The light from the tail gave a continuous spectrum, without superimposed bright bands.

From these results we may draw the following conclusions:

(1.) From the bright bands observed we may infer that the coma consisted in a large measure of matter in the gaseous state; and that the actual gaseous substance was either dioxide of carbon (i. e. carbonic acid gas), or oxide of carbon, or the vapor of some hydro-carbon.

(2.) The light of incandescence of the gaseous particles, which furnished the bands, must have been of electric origin; since the heat of the sun could not have been sufficient to ignite the most inflammable vapor.

(3.) The continuous spectrum on which the three carbon bands were seen, affords no decisive evidence of the presence in the coma of discrete solid particles, since it may have resulted from the solar light reflected from the gaseous particles. Such light would not have been sufficiently intense to give the dark solar lines.

(4.) The "traces of bright bands" seen in the spectrum of the nucleus reveal the presence of vapors at its surface shining by electric light. The bright continuous spectrum may have been wholly due to reflected solar light (since dark solar lines were not wanting), or partly to discrete solid particles rendered luminous by electric discharges. The light reflected from the solid nucleus, or from dense vapors or clouds near its surface, may well have been of sufficient intensity to make the gaseous carbon bands resulting from electric discharges inconspicuous.

(5.) The spectroscope did not give any decisive evidence with regard to the state of the matter in the tail—whether gaseous or composed more or less of discrete solid particles; but since the tail was formed of matter flowing in continuous streams from the head, we must suppose that it was made up chiefly of gaseous particles, like the head.

(6.) The light of the tail was exclusively reflected solar light.

The experiments of Professor Arthur W. Wright, of Yale College, on the gases from stony meteorites,* have furnished strong evidence in support of the hypothesis first propounded by him, that the cometic substance is gaseous carbon dioxide. He found that "in meteorites of the stony kind, the characteristic gas is carbon dioxide, and this, with a small proportion of carbonic oxide (oxide of carbon), makes up more than nine-tenths of the gas given off at the temperature of boiling water, and about half that evolved at a low red heat." The spectrum of the gases, obtained by passing an electric spark through a small tube containing the gases at a low tension, consisted of the hydrogen and carbon spectra together. The three bright bands of the carbon spectrum were coincident in position with those in the spectra of comets, and had the same relative order of intensity. The close relationship now known to subsist between comets and meteors renders it highly probable, as suggested by Professor Wright, that the cometic matter is identical with the gaseous matter found associated with stony meteorites, and consists chiefly of carbon dioxide disengaged from the nucleus of the comet by the heat of the sun.

If we adopt this hypothesis with regard to the nature and origin of the cometic substance, the question arises in what condition does the carbon dioxide exist, in its association with the matter of the nucleus? We may at once admit, with Professor Wright, the high probability that it has become intimately associated with it by interpenetration, or by surface concentration upon small metallic grains, as it is found in meteoric stones. But it is also to be observed, that at the temperature of free space, which must prevail on most comets throughout the greater part of their periods of revolution, a large accumulation of carbon-dioxide might exist on their surface in the solid condition, or as a layer of detached solid particles. This may be inferred from the results of Faraday's experiments on the condensation of this gas into a liquid at extremely low temperatures. He found at the temperature of 80° C., below the freezing point, a pressure of one atmosphere sufficed to condense it into a liquid. 80° C. is, according to Fourier, 20° C. below the temperature of free space, but, according to the more reliable determination of Pouillett 60° C. above it. At this temperature, −140° C., a small fraction of one atmosphere of gaseous pressure should suffice to condense the gas into a liquid.

Now, when a comet, supposed to have an accumulation of solid carbonic acid at the surface of the nucleus, is in the act of approaching the sun, the increasing amount of heat received from the sun should give rise to copious evolutions of the

* This Journal, III, vol. ix, July, 1875, p. 44.

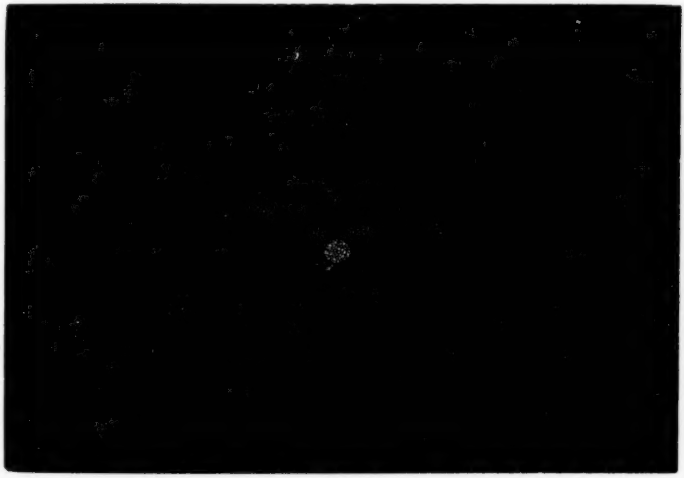
carbon-dioxide in the gaseous form—either continuously or intermittently—and these may occur either simultaneously over large areas, or in limited streams or jets. A portion of the vapor evolved may be condensed into solid particles by the cold resulting from the rapid evaporation. In so far as the gas is in intimate physical association with the solid matter of the nucleus, it would seem that the heat of the sun would not at the perihelion distance of either Coggia's Comet, the great comet of 1861, Donati's Comet, or, indeed, of any of the conspicuous comets with a few exceptions, be intense enough to occasion such copious evolutions of gaseous matter as have been actually observed. Evolutions of occluded gas may, however, constitute the chief phenomena, produced by the solar heat, in the cases of the inconspicuous comets of short period, which, in retreating from the sun do not pass beyond the limits of the planetary system.

The conclusions that have now been reached apply strictly only to the comets that have been spectroscopically observed, but they may be regarded as probably applicable also to other comets that do not differ greatly from these in the circumstances of their approach to the sun and recess from him, and in the physical phenomena they have presented. The great comets of 1843 and 1860, that approached very near the sun, may have given off, in great abundance, aqueous or other forms of vaporous matter, derived from the liquefaction and subsequent vaporization of frozen liquids.

The important question here presents itself, what is the probable origin of the free electricity that plays so important a part in the luminous phenomena which conspicuous comets have presented? We might rest on the assumption that the solid nucleus of a comet is surrounded by a gaseous atmosphere like the earth, permeated by free electricity increasing in tension from the surface of the nucleus upward; and that whatever may be the unknown origin of the electric state of the earth's atmosphere, the same may be the origin of the atmospheric electricity of the comet. But, I will add, from my special molecular stand-point, that the ethereal electric atmospheres by which, as I conceive, the gaseous or vaporous atoms ascending from the nucleus are surrounded, should expand as these atoms recede from the nucleus, and come under diminished gaseous pressure, and so give off a certain overplus of free electricity. The outward flow of such disengaged electricity along the lines of best vaporous conduction should disengage more or less light.

Physical Aspects of the Comet.—From numerous published drawings of the comet, I select that in which its peculiar features are most conspicuous. The cut is a copy of a drawing

communicated by R. S. Newall to A. C. Ranyard, showing the aspect of the comet, as seen at Ferndene on July 12th. In the accompanying description* it is stated that "the nucleus was very bright, with a disk tolerably well defined. In front of the nucleus (i. e. on the side toward the sun) was a fan-shaped light which seemed to arise from the overlapping or duplication of the two tails, which streamed away behind (the nucleus) for a length of about 15°, forming, as it were, two luminous veils, delicate, transparent and flickering, having between them a black space well defined up to the nucleus. The edges of these tails appeared to be brighter than the middle part, and crossing over the nucleus they formed the sides of the fan; the outside edges also crossing over formed the top of the fan and head of the comet. In front of this was another covering semicircular and brightest in the preceding part, and in front of that was again another fainter envelope or cloud." This outer faint envelope, or duplex envelope, has been a noticeable feature in the aspect of other comets (e. g. Donati's).



1.

Physical structure and condition of the Comet.—Upon the various drawings made by different observers, Mr. Ranyard has the following remarks:† "The drawings that were made of Coggia's comet during the early part of July, 1874, show that although there was but one small almost stellar nucleus, *there were two sets of parabolic envelopes situated side by side, and apparently overlapping one another just in front of the nucleus.* These

* Month. Notices of Astr. Soc., 1875–6, p. 279. † Ibid.

were shown in the drawings made by Mr. Huggins and Mr. Christie. They are also to be seen in Mrs. Newall's drawing (see cut, p. 165), and they were described by Mr. Lockyer in a letter published in the Times, of July 16th, on the structure of the comet. When the comet was again visible in the Southern hemisphere, the inner duplicate structure was still visible, but the outer arcs had been dissipated." The *duplicate structure* here referred to is a highly significant fact. That it may be duly appreciated it must be borne in mind that at the period when this structure was observed, the line of sight from the earth to the nucleus was inclined under a small angle to the plane of the orbit, and under a large angle to a line through the nucleus perpendicular to the plane of the orbit. A few days later (July 21) the earth was in the plane of the orbit; the line of sight to the nucleus was in this plane, and perpendicular to the line just mentioned. We must, therefore, infer that at the period when the observations were made *the luminous jets which made up the structure observed were situated in the plane through the radius vector perpendicular to the plane of the orbit, or were seen as projected on this plane.* The lateral dispersion and lines of jet-discharge shown in the drawings, must then have been in the direction of this plane, and not in that of the plane of the orbit. Shall we then conclude that the head and first portions of the tail were flat, or approximately so, and consisted entirely of jets lying in this plane or but moderately inclined to it, or regard the drawings as showing sections of a hollow paraboloid by this plane. We must adopt the former hypothesis, since the strong contrast between the dark space behind the nucleus and the two comparatively broad bright streams on either side of it, and the sharp lines of demarcation between them cannot be reconciled with the latter.

This conclusion suggests at once the inference that the nucleus of the comet must have rotated about an axis approximately at right angles to the plane of the orbit, and that the outstreaming from diverse points of the surface, had some physical relation to the latitudes of these points (i. e. their angular distance from the plane of the orbit or equator). This idea being once admitted we readily perceive that we have a plausible explanation of the duplicate structure of the comet in the hypothesis that separate streams, or systems of jets, proceeded from the two hemispheres lying on opposite sides of the plane of the orbit; and that the overlapping of the envelopes just in front of the nucleus, signalized by Mr. Ranyard, may have been the result of an initial inclination toward the radius vector of the jets issuing from each hemisphere. The prevalent notion that the outstreaming is everywhere in directions normal to the surface of the nucleus, and decreases in intensity

from the region exposed to the normal incidence of the sun's rays, it appears, then, altogether fails of application to Coggia's comet. It gives the single hollow paraboloidal tail without duplication, which is entirely at variance with the facts of observation.

Physical Theory of cometary phenomena.—Now, if there be, in fact, two systems of jets, eminating from opposite hemispheres of the nucleus, and passing over from one to the other, we can look for the origin of such a state of things only in a supposed magnetic condition of the nucleus, and in the hypothesis that the lines of initial discharge lie in the direction of the lines of the magnetic force; or in lines having a certain relation to these. This consideration brings us to the proper point of view for the presentation of the definite physical theory of cometary phenomena which I have been led to form, from a detailed study of Coggia's comet and other comets. It is briefly this: that the direct effect of the action of the sun on the side of the nucleus exposed to the solar rays, is to form an envelope of gaseous carbon dioxide extending a certain distance from the nucleus. This envelope, consisting of a diamagnetic gas, is traversed by the ideal lines of magnetic force proceeding from the nucleus, which are also lines of electric conduction through the diamagnetic gas. The electricity set free by the ascending currents of the gas, by reason of the diminished gaseous pressure, is propagated along these lines; and the impulsive force of the electric currents detaches streams of successive molecules of the gas, in the direction of the lines of conduction. De La Rive's well-known experiment of transmitting electricity through an attenuated gas or vapor surrounding a magnet, showed that the lines of force in the magnetic field were also lines of electric conduction, rendered luminous by the propagated electricity. The outstreaming cometic matter moves away subject to the combined action of the nucleus and sun. Both bodies exert repulsive forces upon the escaping molecules, the probable origin of which I shall consider soon. But their effective actions may be either repulsive or attractive, according as the repulsion prevails over the attraction of gravitation, or the reverse. The repulsion of the nucleus takes effect in directions normal to its surface, or nowhere deviating much from this direction. It should be observed that besides taking effect on the gaseous molecules detached by the electric discharges, it may operate on other molecules not thus detached, with an intensity sufficient to overcome their gravitation toward the nucleus.

In my mathematical discussion of Donati's comet* I reached the result that the tail of the comet was made up of matter of which a portion was solicited by an effective solar repulsion,

* This Journal, II, vol. xxxii, No. 94, July, 1861.

varying in intensity from 0 to 1·213 A (A representing the intensity of the sun's attraction of gravitation at the same distance); and another portion was subject to an effective solar attraction, varying between the limits 0 and 0·455 A. I also showed that the "columnar structure" of the tail, signalized by Prof. Bond, was attributable to considerable variations, at short intervals, in the quantity of matter detached from the head of the comet; while the limits of variation of the effective solar force remained sensibly the same for several days. The varying intensity of the solar repulsion, in its operation on different molecules or particles escaping from the head, was the determining cause of their subsequent wide lateral dispersion in the plane of the orbit. Professor Bredichin, Director of the Observatory of the Moscow University, in a memoir on Coggia's comet, assigns for the effective solar force in operation on that comet, average attractive values varying from 0·118 A to 0·530 A. The values obtained in single determinations come between the limits, repulsion = 1·415 A and attraction = 0·736 A.

Force of Cosmical Repulsion.—Several hypotheses have been propounded with regard to the nature and origin of this force. But none of them appear to be free from serious objections. Several years since (1861) I suggested the hypothesis that the solar repulsion might consist in the repulsive action of free statical electricity. We have abundant evidence of electric excitation both at the surface of the sun and in the cometary envelopes; but it may reasonably be doubted whether the free electricity at the sun's surface can exert, at cosmical distances, an energy as great as that which the operative cause of the wide dispersion of cometic matter displays—especially when it is considered, as suggested by Dr. W. Zenker, that a positive electric state at or near the surface of the sun implies a corresponding negative state at some lower depth.* While not prepared to admit the impossibility of a sensible electric repulsion, it appears to me that another and much more probable view may be taken of the character and origin of the solar force. This is that it is a *diamagnetic repulsion exerted by the sun as an electro-magnet† on the gaseous molecules of the comet.*

* Prof. F. Zöllner, in two elaborate papers published in the Astronomische Nachrichten, No. 2057–2060 and No. 2082–2086, has endeavored to remove the force of the several objections urged by Dr. Zenker to the electric theory of the solar repulsion, and establish the adequacy of this theory. Dr. Zenker has published a detailed reply to Prof. Zöllner's arguments. As one result of the discussion, it does not appear that the above-mentioned objection has been set aside. It must be admitted, I think, that at present the electric theory rests under a cloud of doubt. As for Dr. Zenker's own reaction theory, to mention no other objections, it is certainly wholly inapplicable to the case of a comet coming as near the sun as did the comets of 1843 and 1860; and it obviously affords no explanation of the duplicate structure of Coggia's comet.

† By the term electro-magnet is meant a magnet which derives its magnetic condition from the continued operation of some external cause.

It was conclusively established by Faraday, by a series of careful experiments, that the gases, with the exception of oxygen, are diamagnetic; that is, that their molecules are repelled by both poles of a magnet. That the body of the sun is in a state of magnetic excitation there is abundant evidence. In a former publication * I have undertaken to show that the electric currents in which this must consist, may be ascribed to the impulsive action of the ether of space on the ethereal atmospheres of its molecules, resulting from the combined motion of rotation and progression. If the nucleus of a comet has a motion of rotation, as well as of revolution, the same operative cause should make it an effective magnet. It should accordingly diamagnetically repel the molecules of any diamagnetic gas or vapor that may be posited at or near its surface. The force thus exerted should increase in intensity with the augmenting magnetic intensity, up to the time of the perihelion passage, when the orbital velocity is the greatest; and subsequently decrease. The maximum of magnetic intensity may, however, be reached a certain interval of time after the perihelion passage, by reason of the persistence of currents previously developed. The magnetic condition of the nucleus would be a rapidly changing one, in approximate correspondence with the varying rate of the orbital motion. The circular magnetic currents developed would have diverse directions, in planes lying between the plane of the equator and the plane of the orbit. The effective force exerted by the nucleus on a diamagnetic gaseous molecule, would be the difference between its diamagnetic repulsion and its attraction of gravitation. The solar force of diamagnetic repulsion should vary according to the inverse square of the distance. The mathematical discussion of Donati's comet, already alluded to, served to establish that the effective solar force taking effect on the particles of the tail of the comet varied according to this law. From this fact we may infer that it probably consists of radial impulses propagated in waves through the ether of space.

Inequality of the Solar Repulsion.—Faraday established that the diamagnetism of a gas increased with its temperature. But the separate gaseous molecules of a comet being equally exposed to the sun's rays could not differ materially in temperature. Any single molecule, in receding from the sun, should, however, experience a gradual diminution of temperature, and therefore become less diamagnetic. The inequality of the solar repulsion, taking effect on different gaseous molecules, cannot then be ascribed to changes of diamagnetic condition produced by changes of temperature. But may not material variations of diamagnetic condition result from the electric discharges to which the gaseous molecules are exposed?

Looking at the matter from the general point of view I have taken in my papers on Molecular Physics, it appears that such dis-

* This Journal, II, vol. xli, Jan., 1866.

charges should condense the electric atmospheres (or envelopes) of the molecules, and so augment their absorptive action on the radial impulses of the diamagnetic repulsion. In proportion as the condition of the molecular atmospheres is favorable to such absorption, will the condition of the molecules be unfavorable to the reception of an impulsive action from the diamagnetic force. The tendency of electric discharges should then be to diminish the molecular susceptibility to diamagnetic repulsion from the sun or nucleus of the comet. The varying intensity of such discharges might well occasion large variations in this susceptibility. While a portion of these effects should pass off, as their orignating cause ceases to operate, a certain portion may abide as permanent changes of molecular condition. This is a legitimate deduction from my general molecular theory; from which the variability of the physical condition of the ultimate molecules under varying conditions, is one prominent inference, and the key to the satisfactory explanation of a multitude of phenomena.

The only other view that can be reasonably entertained of a possible origin of an inequality of solar repulsion, is that the cometic particles may differ greatly in size or mass. This might be allowed if we could admit that they are solid particles, or that a large number of different gases are present in the coma. But both of these suppositions seem to be irreconcilable with certain facts of observation.

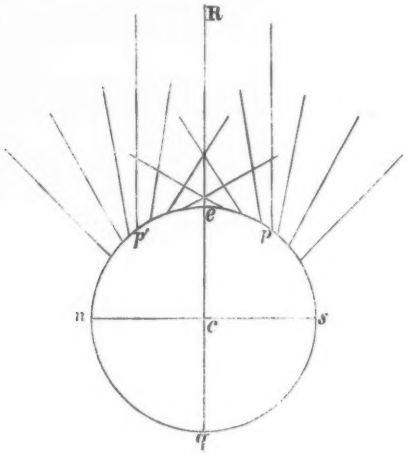
Explanation of General and Special Phenomena.—It should be understood at the outset, that it is not essential to the explanation of cometary phenomena, that the force of cosmical repulsion be regarded as having a diamagnetic origin—that is, as I view the matter, consists of radial impulses propagated in ethereal waves proceeding from all points of the magnetic currents of the cosmical body. The essential feature is that a repulsion, whatever may be its origin, is exerted normally, or approximately so, to the surface of the body, and that the radial impulses of this force take effect unequally on different gaseous molecules; by reason of unequal absorption, resulting probably from certain effects produced by electric discharges—but possibly on solid cometic particles differing in size or mass. A magnetic condition of the nucleus, sufficiently decided to determine lines of electric conduction through its diamagnetic gaseous envelope, coincident with the lines of force in the magnetic field, is however an essential feature of the theory.

The precise character of the phenomena should depend, to some extent, on the position of the axis of rotation of the nucleus. We may regard as the normal position that in which it is perpendicular to the plane of the orbit; and the attendant phenomena as the general phenomena. If the axis deviates from this position there will be two efficient causes of special phenomena to be considered: (1) the point of maximum evap-

orating effect of the sun's rays will probably fall at some point of either the northern or the southern hemisphere of the nucleus, instead of on the equator; (2) the magnetic poles will not coincide with the poles of rotation, and the magnetic equator will be more or less inclined to the plane of the equator of rotation, as well as to that of the orbit. From this it follows that the lines of force will, in general, be more or less inclined to the astronomical meridian planes of the nucleus; and hence that the initial directions of the jets of cometic matter will be inclined to these planes.

Let us first take the axis in its normal position. We at once perceive that there may be two general varieties in the process of evaporation under the influence of the sun's rays. (1.) A continuous evaporation most abundant at the equator, and extending into both hemispheres. (2.) A more copious evaporation occurring only at certain intervals, beginning in each instance at the equator, and subsequently extending both north and south. A tendency to such intermittence at each point of a meridian should result from the cold produced by a copious evaporation and the intercepting action of the vapors already generated. The vapor or gas developed by either of these special processes becomes subject to an expulsion to an indefinite distance, either by an effective repulsion exerted by the nucleus, or by the impulsive force of electric discharges combined with the effective force exerted by the nucleus, whether attractive or repulsive. In both of these modes of expulsion the gaseous molecules, in their motion *relative to the nucleus*, are subject to the full force of the sun's repulsion, undiminished by his attraction of gravitation. Those which experience the first mode of expulsion, not having suffered any diminution of susceptibility to diamagnetic repulsion by reason of electric discharges (p. 170), are energetically repelled both by the nucleus and sun. They become separately visible in the straight "secondary tails" (e. g. Donati's comet). On the other hand, the gaseous matter expelled by electric discharges, serves to form the envelopes that rise in succession from the nucleus (e. g. Donati's comet; great comet of 1861). In the formation and expulsion of each successive envelope, whichever of the two processes of evaporation may occur, the evolution of gaseous matter will be most abundant at the equator, and electric discharges may reasonably be supposed to begin near the equator, and extend gradually both north and south. The varying directions of the discharges, and therefore also of the initial directions of the jets, from diverse points of a single meridian, are shown in Fig. 2. At about 35° from the equator, on either side, the direction becomes parallel to the equator (*ecq*), and thus to the radius vector (*ceR*). From 0° to 35° it tends toward

this line, and beyond 35° diverges from it under a larger and larger angle. If the initial velocity is constant, as well as the solar repulsion, the escaping molecule should attain to its greatest distance from the nucleus when the discharge occurs from the latitude of 35°, and in the precise direction of the sun. If then the process of electric discharge begin near the equator, and extend gradually to the north and south, the outer surface of the envelope formed will gradually move away from the nucleus, and attain its greatest distance when the process reaches the latitude 35°. The jets that issue from latitudes greater than that (about 35°) at which the direction is parallel to the radius vector, do not pass sensibly beyond the boundary line of the jets that proceed from points between 0° and 35° of the other hemisphere, unless for such jets the projectile velocity is greater, or the solar repulsion less.*



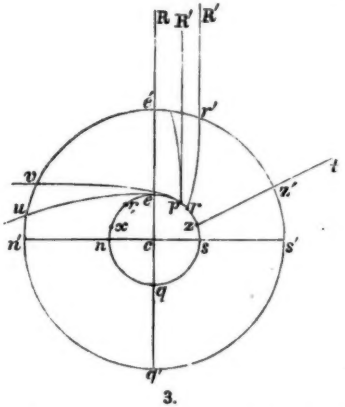
2.

What has now been stated should be the precise result if the molecules after receiving a projectile velocity were exposed only to the retarding force of the solar repulsion. The effective action of the nucleus would obviously modify somewhat the curve for each initial direction, and alter the limiting latitude for which the recess in the assumed direction is a maximum. If this effective action is attractive, this latitude will exceed 35°, if it is repulsive, it will be less than 35°.

We may infer from the great dimensions of the head of a comet in comparison with the nucleus, that the action of the nucleus becomes practically insensible at a small fraction of the

* See remark about recess of envelopes on p. 176.

distance to which the jets recede in the direction of the sun. We may then regard any expelled particle as issuing from the small sphere of sensible action of the nucleus with an initial velocity resulting from the projectile force of the electric discharge, and the retarding force of the nucleus (or accelerating force if the effective action should be repulsive); and as subsequently retarded by the solar repulsion. As we have seen, p. 170, the gaseous molecule which receives the greatest electric impulse should thereafter be the least susceptible to repulsion by the sun or nucleus. Such a molecule, though receiving the highest projectile velocity, should then be most retarded by the effective attraction of the nucleus (or least accelerated by its effective repulsion). The tendency of this state of things is then, to bring the velocities of all the different molecules, as they emerge from the sphere of sensible action of the nucleus, to approximately the same value. In considering the subsequent motion of a molecule, under the retarding influence of the solar repulsion, its velocity and direction of emergence from this sphere, becomes the *initial* velocity and direction.

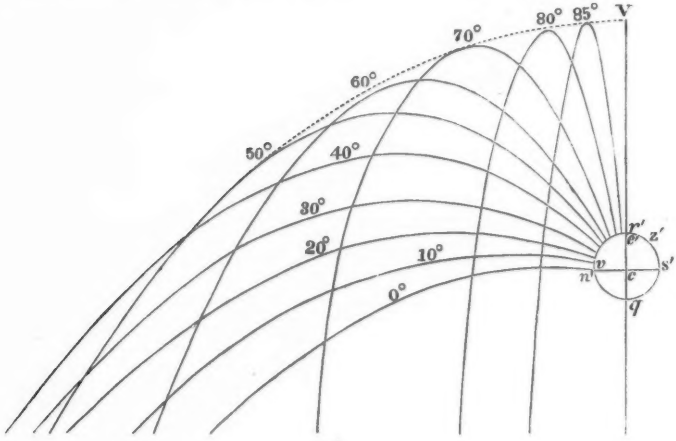


3.

As, for any one molecule, the solar repulsion is sensibly constant within the extent of the head of the comet, the path of the molecule will be parabolic. If we regard the initial velocity as constant for the different molecules, those which suffer the least retardation from the solar repulsion should attain to the greatest distance from the nucleus; and at the time of greatest recess lie in the outer surface of the envelope. Those which suffer the greatest retardation should form a similar luminous surface at a lower depth. The latter would eventually form the preceding side of the tail of the comet, from a certain distance behind the nucleus, while the former would

form the following side. The time required for any receding particle receiving an initial velocity in the line of direction of the sun, to reach its point of greatest recess, must have been less than twenty-four hours, in the case of the two comets (Donati's and Coggia's) for which the intensity of the actual solar repulsion has been determined.

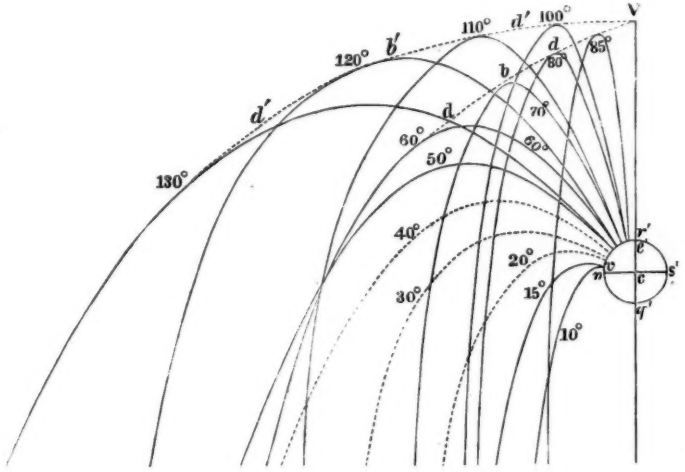
Let $n\,e\,s\,q$ (Fig. 3) represent the nucleus, or more strictly the surface of projectile discharge, and $n'\,e'\,s'\,q'$ the sphere of sensible action of the nucleus; $e\,q$ being the equator and $c\,e\,R$ the direction of the sun. The point p is at lat. 35°, and r is the point of projection of the jet which issues from the sphere $n'\,e'\,s'\,q'$ in the direction $r'\,R'$ parallel to $c\,e\,R$. The jets proceeding from the arc $e\,r$ issue from this sphere at various points from u to r'. One of these jets proceeding from a certain point near e will have at v a direction at right angles to $c\,e\,R$.



4.

If the jet discharge extend beyond r to some point z the jet proceeding from z will emerge from the sphere $n'\,e'\,s'\,q'$ at z', in a direction $z'\,t$, having a certain inclination to $c\,e\,R$. The cometic matter outstreaming from the arc $r\,z$ will emerge from the same sphere between r' and z'. Now let $n'\,e'\,s'\,q'$ (Fig. 4) answer to $n'\,e'\,s'\,q'$ in Fig. 3, and let a denote the inclination of any jet emerging from this sphere to the line $n'\,c\,s'$ perpendicular to the radius vector $c\,e'\,R$. Suppose a (estimated from left to right) to increase by 10° from 0° to 90°, and let us regard the initial velocity, on leaving the sphere $n'\,e'\,s'\,q'$, as constant, (p. 173) and also the solar repulsion as constant for each jet. The parabolic curves that would be pursued are

shown in the diagram. The jets proceed from $n' v e' r'$, and were projected from the lower latitudes of the right hemisphere, or between e and r (Fig. 3). Jets issuing from the corresponding arc $e r_,$ of the other hemisphere, would form a similar set of curves to the right of $c e' R$. Other sets of parabolic jets are projected from latitudes above 35°, or from the arcs $r z$ and $r_, x$ (Fig. 3); but these will have sensibly the same outer boundary as those from $e r'$ and $e r_,$ unless the initial velocity or the solar repulsion is different. It is to be observed that $c e$ is so small a fraction of $c e'$ (Fig. 3) and $c e'$ so small a fraction of c V (Fig. 4) that we may without material error regard v as coincident with n', and r' as coincident with e'.



5.

The hypothesis made in the construction of Fig. 4, that the solar repulsion is constant for all values of a, or in other words that the force of electric discharge on which the intensity of this repulsion theoretically depends, is independent of the initial angle of direction of the jet, is not the most probable one under the circumstances. In fact, the electric discharges at the nucleus should apparently increase in intensity and impulsive action, with the inclination of the line of discharge to the surface, and hence the solar repulsion should, according to the theoretical principle before stated (p. 170) diminish as this angle of inclination increases. This angle corresponds to the dip of a magnetic needle on the earth's surface, and varies, we may suppose, with the latitude according to a similar law. Let it be denoted by β.

In Fig. 5 the solar repulsion is assumed to be inversely proportional to sin β. The change of direction of the jet while passing through the sphere of sensible action of the nucleus is neglected in the application of this law. Two systems of curvilinear jets are shown—one answering to values of a varying by 10° from 50° to 90°, and emanating from latitudes less than 35° of the right hemisphere, or from a portion of the arc $e\,r$ (Fig. 3); and another answering to values of a varying by 10° from 90° to 130°, and estimated from right to left. These emanated from latitudes greater than 35° of the left hemisphere, or from a portion of the arc $r,\,x$ (Fig. 3). The other corresponding systems of jets, emanating from portions of $e\,r,$ and $r\,z$, would form the other half of the comet. If we consider all the jets issuing from the right hemisphere as forming one system, and all those issuing from the left hemisphere as forming another system, then for each system a will vary from 50° to 130°: and will increase from left to right for the first, and from right to left for the second. They answer to the supposition that the outstreaming extends from latitude $17\frac{1}{2}$° to $57\frac{1}{2}$°, in each hemisphere. It will be seen that the jets proceeding from the latitudes higher than 35°, for which a varies from 90° to 130°, recede to greater distances from the nucleus than those emanating from latitudes less than 35°, for which a varies from 50° to 90°. This of course results from the greater values of sin β that obtain for the first-mentioned set of jets. Thus, for the 130° jet sin β=0·953, while for the 50° jet sin β=0·534. The envelope, $V\,a\,b\,d$, or outer boundary of the latter system of jets thus falls within that, $V\,a'\,b'\,d'$, of the other system. The overlapping of the two systems of jets should accordingly be conspicuously visible, and an appearance presented similar to that of Coggia's comet on July 12th, as shown in the drawing on page 165.

This drawing indicates that the evolution of cometic matter was much less for angles a from 0° to about 50° than from 50° to 130°. Not to confuse the drawing too much I have shown in full lines only two jets between 0° and 50°, answering respectively to 10° and 15°; as showing that the boundaries of the dark space behind the nucleus were probably formed by jets issuing from low latitudes on the nucleus.

It ought to be added, as an important feature of the general theory, that the recess of a cometary envelope, or system of jets from the nucleus, should continue after the jet discharge has reached the latitude 35° and upward, until the electric discharges at these latitudes have attained to the maximum intensity.

Certain comets have presented peculiarities of appearance which I find, on a careful examination, admit of satisfactory explanation on the hypothesis that the equator of the nucleus was inclined to

the plane of the orbit, and in consequence the two magnetic hemispheres were unequally exposed to the influence of the sun's rays. The special features alluded to, are the unsymmetrical position of the envelopes thrown off in succession from the nucleus (e. g., the envelopes of Donati's comet in their earlier stages, and especially the five separate envelopes of the great comet of 1861, as seen by Dr. Schmidt on June 30th); and great differences in the length of the two branches of the normal tail, in connection with an anomalous curvature of the first portion of the longer branch, when the comet was viewed from certain positions of the earth relative to the plane of the orbit, observed in the case of Comet II, 1862, and elaborately discussed by Prof. Schiaparelli and Prof. Bredichin. In this case we have only to suppose that the sun was vertical to points of one of the hemispheres, and as a consequence the jet discharges were mostly confined to that hemisphere. The longer branch of the tail was composed of jets issuing from the lower latitudes, while the less copious and more fluctuating discharges of matter subject to a diminished solar repulsion (p. 176) from the higher latitudes, formed the shorter branch of variable length.* The anomalous curvature of the former system of jets, and their interlacing with the other system, was a simple consequence of the greater intensity of the solar repulsion in operation on the former than on the latter.

The curious phenomenon of the *oscillation of jets* first observed by Bessel in the head of Halley's comet, and of which he offered in explanation the improbable hypothesis of a polar attractive force exercised by the sun upon the nearer portion of the nucleus, may be seen to be another probable consequence of an inclination of the equator to the plane of the orbit. It has already been intimated (p. 171), as one theoretical result of such a state of things, that there should be two magnetic poles in each hemisphere, and that the lines of magnetic force should be variously inclined to the planes of different local meridians on the nucleus. As the planes of these different local meridians are brought successively by the rotation into coincidence with the meridian plane through the sun, the lines of jet discharge, which are coincident with the lines of magnetic force, should oscillate with respect to this plane.

* To illustrate, if the outstreaming were confined to the right hemisphere, (Fig. 3) the jets proceeding from *e r* would pass over to the left side of the nucleus, and those proceeding from *r z* would curve around to the other side.

ART. XXII.—*On the Velocity of Transmission of Earth Waves;* by General H. L. ABBOT, Corps of Engineers.

ADVANTAGE was taken of the explosion of 50,000 pounds of dynamite at Hallet's Point, on September 24th, 1876, to measure the velocity with which the shock was transmitted through the ground, both across Long Island and along the south bank of East River. The results were embodied in a paper read by me before the National Academy of Sciences, on October 18th, 1876, and subsequently again read and printed as one of the papers of the Essayons Club of the Corps of Engineers.

In the number of the London, Edinburgh and Dublin Philosophical Magazine, for October, 1877, appeared a short review of this paper from the pen of Mr. Robert Mallet, F.R.S., a gentleman well known for his numerous and able contributions to seismology. In this article, he suggested reasons which led him to doubt the value and accuracy of the Hallet's Point results.

Even if I had felt disposed to enter into a controversy upon the subject, I should have been quite disarmed by the concluding sentence of this article, which expresses views so just and liberal that it may well be quoted as an exemplar of the manner in which scientific questions should be considered. He writes:

"In these objections I wish to be clearly understood as having no *a priori* difficulty in accepting a higher velocity of wave transit than the highest attained experimentally by myself. It is highly probable that such may be elicited by future experiment. But should such cases arise, their results like all great physical truths, should only be credited upon unexceptionable observations or experimental evidence. While feeling justified in making these objections, I wish to disclaim all controversial spirit or intention; loss of sight, indeed, and diminished energy would prevent my engaging in any scientific controversy, were any called for."

Believing, at the date of my first paper, that the data secured at the Hallet's Point explosion demonstrated the necessity for more exact and comprehensive knowledge of the subject, I have, during the past season, taken advantage of the facilities offered by large sub-aqueous explosions at the School of Submarine Mining at Willet's Point, to continue the investigation; and, on October 23d, 1877, I read a second paper before the National Academy of Sciences, giving the results thus obtained. As only a brief abstract of this paper has appeared in print, I propose now to give a summary of the conclusions suggested by the whole series of experiments and, incidentally, to explain

my reasons for believing that Mr. Mallet has not quite understood the parts of my first paper to which he has taken exception.

Limited space forbids any detailed explanation here of the method adopted for measuring the time of transmission of the shocks; especially as this is fully given in my printed paper, together with the notes of the observers in full. Suffice it to say that the instant of explosion and the time of arrival of the tremors, were electrically recorded on the same moving paper with extreme precision. The following table exhibits the data, of which only the first six observations were known to Mr. Mallet when he wrote his article.

No. of observations.	Date.	Observer.	Cause of shock.	Distance to station.	Type of seismometer.	Tremor of mercury.		Velocity of transmission.
						Arrived in	Lasted for	
				miles.		secs.	secs.	ft. per sec.
1	Aug. 18, '76.	Capt. Livermore	200 lbs. dynm.	5 ±	B.	5 ±		5280 ±
2	Sept. 24, '76.	Lieut. Young	Hallet's Pt. ex.	5·134	A.	7 ±	63 ±	3873 ±
3	" " "	Lieut. Griffin	" " "	8·330	B.	5·3	72·3	8300
4	" " "	Lieut. Kingman	" " "	9·333	A.	10·9	23·5	4521
5	" " "	Lieut. Leach	" " "	12·769	B.	12·7	19·0	5309
6	Oct. 10, '76.	Lieut. Kingman	70 lbs. powder.	1·360	A.	5·8	inst.	1240
7	Sept. 6, '77.	Lieut. Kingman	400 lbs. dynm.	1·169	A.	1·8	7·8	3428
8	" " "	Lieut. Leach	" " "	1·169	B.	0·7	17·8	8814
9	Sept. 12, '77.	Lieut. Griffin	200 lbs. dynm.	1·340	A.	1·05	8·8	6730
10	" " "	Lieut. Leach	" " "	1·340	B.	0·81	17·1	8730
11	" " "	Lieut. Griffin	70 lbs. powder.	1·340	A.	1·27	4·8	5559
12	" " "	Lieut. Leach	" " "	1·340	B.	0·84	15·1	8415

Mr. Mallet's results were reported many years ago to the Royal Society, and unquestionably are to be accepted as exhibiting the velocities due to his initial shocks and given by the method which he employed. Unfortunately, no full description of this method, and especially no record of the optical power of the instrument, is contained in his paper to the Royal Society; and my results, as will soon appear, prove the latter to be of unexpected importance. The following are his figures:

Velocity in ft. per second in sand ..... 825 ft.
" " " in discontinuous and much shattered granite ..... 1306 ft.
" " " in more solid granite ..... 1665 ft.
" " " in quarries at Holyhead (mean) 1320 ft.

The extraordinary differences between these rates and those measured at the Hallet's Point explosion, and the apparent discrepancies of the latter among themselves, led me to so plan the new observations as to throw light upon two points: 1st, Does a telescope of high power detect a tremor in the mercury in advance of the one first revealed by a lower power? 2d, Is

there a difference in the rate of transmission of the shocks, due to differences in the intensity of the initial explosion?

The method adopted was to station two observers near each other at a carefully selected inland position; each observing a mercury seismometer, and holding in his hand the key of an accurate Morse register to record the instant of arrival and the duration of the tremor. These seismometers were the same instruments used at the Hallet's Point explosion. They differed from each other only in the optical power of the telescope, that designated A in the table having a magnifying power of 6, and that marked B of 12.

A fuse in the electrical circuit which fired the torpedo, was bedded in the cartridge of a field gun directed toward the observers; and, as a very powerful battery was used, the two explosions were absolutely simultaneous. A third observer, holding in his hand a key which closed the circuit of both registers, observed the muzzle of this gun with a good telescope, and recorded the flash electrically with precision. The observers were all officers of the Corps of Engineers accustomed to delicate observations. The distance traversed by the shock was measured by triangulation.

The observations of September 6th and September 12th, 1877, marked 7 to 12, and sufficiently explained in the foregoing table, answered the query relative to optical power decisively in the affirmative; and, as may be seen from the table, the earlier observations singly and collectively suggest the same conclusion. The mean velocity given by the six observations with type A is 4225 feet per second, while that given by the same number with type B is 7475 feet per second. There can, therefore, be no doubt that the first tremor of the mercury is of too feeble intensity to be detected with a magnifying power of 6. Hence, as the first tremor determines the true rate of transmission of the shock, the rejection of all the observations made with type A follows as a matter of course. They are valuable as exhibiting the rate of advance of waves having a certain intensity, but they do not reveal the velocity of the leading tremor. Indeed it is not impossible that a power above 12 might have detected a still earlier tremor; or, in other words, have indicated velocities of transmission exceeding those in the table.

These conclusions are so novel and important that certain additional evidence will be given, furnished by the notes of the observers recorded at the time and without consultation.

Lieut. Leach, using a power of 12, recorded of the second observation of September 12th (No. 12), "The gunpowder wave was peculiar, in having a much more gradual increase than has been observed in dynamite shocks. I should say it was at least two seconds in attaining a maximum, whereas the dynam-

ite usually reaches its maximum in a very small fraction of a second."

Lieut. Griffin, who used a power of 6, recorded of the same shock (No. 11), "The gunpowder disturbance remained constant for a perceptible interval, instead of immediately decreasing as is the case with dynamite."

It will be noticed that the shock of October 10th, 1876 (No. 6), was caused by the explosion of seventy pounds of gunpowder—the same charge which was fired on September 12th, 1877, and to which these records refer—but that the duration of the tremor was much less. The reason is, that the initial earth waves were far more violent in the latter case, when the torpedo lay on the bottom in thirty feet of water, than in the former, when the charge was only submerged five feet in water thirteen feet deep, and thus expended much of its energy in throwing a huge jet of water 330 feet into the air.

Thus it will be seen that these records, and the velocities observed on the two days, all tend to confirm the idea that a slow-burning explosive, like gunpowder, generates a series of gradually increasing tremors which, at a distance of a mile, are at first quite invisible with the less sensitive seismometer; and are only detected by it when near their maximum intensity. If Lieut. Leach's estimate of time for the arrival of the maximum wave be accepted, we have, therefore, for the first mile:

Deep torpedo (70 lbs.)	Power of 12 gives	8415	feet	per	second.
	" " 6 "	5559	"	"	"
	Estimated minimum power gives	2489	"	"	"
Shallow torpedo (70 lbs.)	actual minimum power gives	1240	"	"	"

Without wishing to attach undue weight to these records, they appear to me to suggest a possible explanation of the discrepancies between the results obtained by Mr. Mallet and myself, provided his magnifying power was low. It would be very interesting to know the actual power employed.

As to the second query—Whether the rate varies with the intensity of the initial shock, the data, although less decisive, certainly suggest an affirmative answer. The evidence afforded by No. 6 and No. 11 has been already pointed out. For a power of 12, the table shows that for the first mile:

400 lbs. of dynamite give	8814	feet	per	second.
200 " " " "	8730	"	"	"
70 " " powder (deep)	8415	"	"	"

If it be admitted also that the velocity of the wave diminishes with its advance, all the data become accordant. Thus:

200	lbs.	of	dynamite	give	for	1	mile,	8730	ft.	per	second.
"	"	"	"	"	"	5	"	5280	"	"	"
50,000	"	"	"	"	"	8	"	8300	"	"	"
"	"	"	"	"	"	13½	"	5300	"	"	"

In fine, I believe that the following general conclusions are suggested by these measurements: 1st, A high magnifying power of telescope is essential in seismometric observations. 2d, The more violent the initial shock the higher is the velocity of transmission. 3d, This velocity diminishes as the general wave advances. 4th, The movements of the earth's crust are complex, consisting of many short waves first increasing and then decreasing in amplitude; and, with a detonating explosive, the interval between the first wave and the maximum wave, at any station, is shorter than with a slow-burning explosive.

A summary of the objections suggested by Mr. Mallet to the Willet's Point observations will now be given; although, in the new light thrown upon the subject by these subsequent measurements, perhaps he might not desire to press them.

Mr. Mallet considers that the explosion at Hallet's Point furnished data ill suited to determine the delicate physical problem of the rate of progression of a wave of disturbance through the earth's crust, for the reason that the initial impulse was due to the combined effect of many small charges, and not of one single mass of dynamite; hence, as he states:

"I conceive it, therefore, impossible with sufficient exactitude to assign the instant at which the wave of shock started in this instance; and to assume the appearance of disturbed water above the seat of explosion, or the shock felt from the ground closely adjacent to it, as marking that instant seems to me, in either choice, to neglect many sources of error."

Being unable to read my paper personally, he has not fully understood the manner in which the instant of the starting of the wave was fixed. The great mine was fired by a mechanical drop, which closed simultaneously all the twenty-three electrical circuits. One extra pin and mercury cup in this drop was provided by General Newton for my use, and the signal was thus automatically sent over the telegraph line to the several stations. Human judgment had no part to perform. The absolute instant of closing the battery circuits was electrically recorded on all the register papers. It is impossible that this could have *followed* the explosion; and if it preceded it, by a differential of time, the effect upon the records would be to indicate *too slow* and not *too fast* a rate, and thus to bring my results more nearly into accordance with those of Mr. Mallet.

Mr. Mallet next argues that the discrepancies of the four records among themselves invalidate their accuracy. I have just shown how these apparent discrepancies are explained by

the later observations at Willet's Point; and have thus answered this argument which, when the results were first collated, suggested itself to my own mind.

Mr. Mallet next mentions certain objections that show him not quite to understand the geography and geology of the region separating Hallet's Point from the four stations; which, in the absence of a map to illustrate the paper, is very natural. The straight line to Willet's Point follows the shore and waters of East River, offering both a land and water route to the wave of disturbance. The other three stations lie in the interior of Long Island, with no water between them and Hallet's Point. The intermediate country consists of low rolling hills formed of deposits of the clay, sand and boulders characteristic of drift, the geological formation to which they belong. The stations were carefully selected to detect, if possible, what influence is exerted upon the rate by intermediate sheets of water; and the fact that the wave reached Willet's Point with a higher velocity than the inland stations is, perhaps, confirmatory of the idea that water is a better conductor than land.

Mr. Mallet refers, as causes of uncertainty, to three sets of waves generated: (1) by the explosion, (2) by the fall of the rock, (3) by the fall of the water; and also to the wave-diffusion experienced in traversing long distances. He argues, therefrom, that the distances were too great for satisfactory observations. Now the records show both the beginning and end of the mercury vibration, as well as the automatic signal sent by the explosion. The first and last determine the velocity of the advanced wave, which is that given in the table; the second and last approximately fix the time of arrival of the last wave, and enable its velocity to be roughly estimated. The physical phenomena following so exceptionally large an initial shock are thus made a matter of record. As to the element of distance, it seems to me that the reduced probable error in the measurement of the time of transit, should give the preference to long over short routes, when the velocity is so high as that under consideration.

As to the precision with which these distances were determined, Mr. Mallet expresses doubt; but since they depend upon the exact triangulation of the Coast Survey, supplemented by careful railroad surveys, no scientific man who is familiar with the geodesy of this region will hesitate for an instant to give them full credence. The probable error due to length of route, as compared with that due to any possible measurement of so short a time, is practically zero.

Mr. Mallet suggests that a measurement of the personal equation of the observers should have been made. This was not done, because absolute, not differential times were to be ob-

served; and no absolute personal equation machine was available. The officers were trained observers; and their distances from Hallet's Point were so considerable, that a small personal equation would probably exert no material influence on the relative velocity over the different routes.

In fine, my own opinion is that in this, as in all physical investigations, the more thoroughly the ground is covered the greater will be the number of important facts developed; and that complex rather than simple phenomena should be expected. It is quite impossible to ascribe the discrepancy between Mr. Mallet's results and those here described to inaccuracy of observation. Differences in the material traversed by the waves, and in the method of observing, may possibly explain them. If Mr. Mallet's health will permit him to publish the details of his mode of observing, and to give the whole subject a general discussion, the paper will certainly find interested readers.

Willet's Point, N. Y. Harbor, Jan. 14, 1878.

ART. XXIII.—*On Systems of Chemical Notation.* Letter of M. BERTHELOT to M. Marignac* (from the Moniteur Scientifique of December, 1877).

ALLOW me, in the first place, to correct the opinions concerning the teaching of chemistry in France, which I find expressed in your article, and which have been propagated, perhaps intentionally, in foreign countries. There is no regulation which makes it obligatory on professors of the Faculties to adopt any particular notation, and, as a matter of fact, both notations, by equivalents and by atoms, are about equally represented in our lectures. At the Sorbonne, MM. Würtz and Friedel have adopted the system of atoms; the College of France presented last year a professor who uses the atomic notation; I was commissioned to make the report to the Minister, who gave his approval and made the nomination. In examinations both notations are equally accepted, and, if any pressure exists on the candidates, it is exerted by the partisans of atoms rather than by the others. Officially, therefore, perfect freedom exists on this question. If the atomic notation has not been generally accepted in France, it is because it has not succeeded, so far, in obtaining the good opinion of the majority of scientists; but, notwithstanding, imputations have not been spared that the partisans of equivalents are animated with a retrograde spirit.

* An answer to the paper in Moniteur Scientifique, September, 1877. (See this Journal, February, 1878, p. 89.

Allow me, in the next place, to point out some observations on the fundamental part of the question under discussion. It presented itself before the Paris Academy of Sciences under two heads: the system of atoms, and the language or notation of atomic weights. You were right in separating these two things. I had tried to do the same thing, but with less distinctness, in my last work, *On Chemical Synthesis*, in which I explained the system very fully, but without adopting it, and I said that the notation by atoms possesses certain advantages, but also some disadvantages. The discussion recently raised could not, in the nature of things, assume this methodical form; but I believe that I kept about the same ground, as I always said that the two languages expressed the same ideas in the same way, in most cases, except that special advantages belonged to each system of notation. Your conclusions seem to be about the same as mine.

The definition of equivalents, which you accuse me of not giving, was nevertheless presented during the discussion, and I will take the liberty of reproducing it: "Equivalents express, in my opinion, the ratios of weight according to which bodies combine or substitute themselves for one another." These ratios may be determined by the balance with infinitely greater precision than can be ascribed to most physical laws. As, however, experience proves that bodies combine according to several proportions, which are multiples of one another, it follows that equivalents themselves are only determined within an approximation of a multiple of a certain unity, precisely as axes are determined in crystallography. The choice of the unity belonging to each body is therefore somewhat arbitrary. It may be determined from purely chemical considerations, which are never wanting, by taking the weight which agrees the best with the general reactions of the body, which affords the simplest form, and that which conforms the best with analogies.

These analogies are generally expressed by precise rules which are founded on the reciprocal substitution of metals and metalloids, the formation of oxides and acids, and their reciprocal combinations, and the multiple proportions according to which elements combine. In only one case, that of alumina, have we had to appeal to more delicate analogies, drawn from the existence of a remarkable class of double salts, which have been corroborated by the general resemblance of the salts of this base with those of the sesquioxides. It is only in a subordinate way, and for the purpose of giving greater precision to chemical analogies, which are often somewhat vague, that physical properties have been introduced, such as the gaseous density, the specific heat, the crystalline form, the molecular volume in the solid state, etc.

The part which physical properties are to play in the determination of equivalents, and the relative importance of these properties seem to me to constitute the only difference between your views and mine. If it was possible to make equivalents agree exactly with gaseous densities, as the old atomic school had hoped to accomplish, the numbers obtained would probably be adopted by all chemists. This has happened in organic chemistry, in which the same equivalent weights are accepted by every body. Unfortunately, however, this concordance does not exist in mineral chemistry, whence the attempt of the new school to establish atomic weights by means of specific heats. But I persist in the opinion, although I am sorry to find that it is opposed to yours, that this base has not sufficient theoretical solidity when it is in contradiction with the gaseous densities. The specific heats of simple gases, which obey the laws of Mariotte and of Gay Lussac (an obedience which is expressed by the constancy of their gaseous densities), are necessarily the same under the same volume, because the specific heat measures the work accomplished in fulfilling these laws. If the specific heats of the elements in the solid state do not observe the same ratios as in the gaseous state, it is for one of the two following reasons, either the specific heats of the solid elements change unequally with the temperature, as I believe is the case, or two gaseous molecules are united in one solid molecule, as the atomists suppose. In either case, it seems to me that the specific heats of solids must be put aside in the determination of absolute equivalents.*

I insist the more on this point that the new equivalents, if we attribute to this word the extensive meaning that you rightly give to it, introduce an undeniable complication in chemical reactions. In your classical researches on the specific heats of saline solution you found yourself obliged to double the atomic weights of hydrochloric and of nitric acid and of their salts, with the object of expressing with greater clearness the analogies and parallelism of their properties. You wrote:

$$H^2Cl^2;\ Na^2Cl^2;\ N^2O^5,\ H^2O;\ N^2O^5,\ K^2O,$$

and in the same manner you were led to double acetic acid and the acetates:

$$C^4H^4O^4,\ H^2O;\ C^4H^4O^4,\ K^2O.$$

The same necessity has been felt by all those who have had to express the equivalent ratios of acids, of water and of bases,

* I cannot accept your opinion on the absolute value of the law of Wœstyn in the calculation of the specific heats of solid compounds. You know very well that M. Kopp, who went to the bottom of this question in 1864, found himself obliged, in verifying this relation, to attribute to the solid elements in their combination specific heats varying from 6·4 (silver, chlorine, nitrogen), down to 4 (oxygen), 2·3 (hydrogen), and 1·8 (carbon).

as may be seen in the remarkable papers of Mr. Thompson on Thermo-chemistry, and even in the new edition of Gmelin, now publishing in Germany (see, among other things, iodic acid I^2O^5, H^2O).

The agreement of the numbers adopted by the partisans of atomic weights is then more apparent than real.

But I have no wish to prolong this controversy, particularly, as, between us, the only question is as to ascribing one value or another to the unity, of which the various equivalents of the same body are multiples. If we confine the question within these limits, it certainly does not present the excessive importance which has been ascribed to it for the last twenty years. The new atomic school has not, it appears to me, justified its pretension of changing the very base of chemical doctrines, and of founding a *new chemistry*, essentially different from the old. The only thing it has done has been to intermix the meshes of its hypotheses with our demonstrated laws, and this to the great detriment of the teaching of positive science. I believe that it would be advisable, in the future, to set aside all these systems, and to turn the minds of young scientists towards the really new views offered by molecular mechanics, which promise such rich harvests of discoveries.

Benzeval-sur-Dives (Calvados), August 10th, 1877.

Answer of M. Marignac.

I cannot but esteem myself very happy that the remarks which I recently presented in the *Moniteur Scientifique* on systems of chemical notations have given rise to the interesting article which precedes. I would like, however, to add a few remarks.

I found this fault with chemical equivalents, that they are not susceptible of a precise and general definition. M. Berthelot, while opposing this doctrine, seems to me to confirm it, as he was obliged to give a double definition. Sometimes these equivalents represent the ratios according to which bodies substitute themselves to one another; this is certainly a precise conception, which I have called *true chemical equivalence*, but it can only be applied to restricted cases. At other times, they are the ratios according to which bodies combine, or rather one of the ratios, which are multiples of one another, according to which combinations take place; in this case, the question of equivalence remains undetermined and more or less arbitrary. Besides, we may add that it is not always easy to see why one or the other of these definitions can be applied to the equivalent of a body. For instance, aluminium, compared to the metals which are most nearly related to it, those of the earths and alkaline earths, has a perfectly determinate value of substitution, and still this is not the value that has been chosen for its equivalent.

As M. Berthelot himself observes, we only differ in the opinion that each of us has formed on the part and relative importance which are to be ascribed to physical properties in the determination of equivalents. I may possibly have an exaggerated idea of the importance of these properties. But does not M. Berthelot, on his side, labor under a delusion when he thinks that chemical considerations alone are a sufficient guide to a chemist in this determination; is he not under the influence, which he has often discovered in his opponents, of a state of mind in which things seem very natural because we are accustomed to accept them? If the equivalent adopted for aluminium was not confirmed by the specific heat of this metal, by the vapor density of its chloride, and by numerous considerations of isomorphism, is he very sure that we would not hesitate between the formulas Al^2O^3 and AlO for alumina? What would make us suppose that hesitation would be permissible is the great number of bodies in which the determination of the equivalent has remained doubtful as long as physical properties have not served as guides, such, for instance, as in silicon, zirconium, glucinum, and the numerous group of the metals of cerite and gadolinite. The only thing I ask is to allow the same importance to physical properties in the case of bodies which occur with frequency.

M. Berthelot finds fault with me for being in apparent contradiction with myself, because in my researches on the specific heats of saline solutions, I doubled the molecular weights of some compounds, to better express the parallelism of the properties of some compound groups. It is true that, in comparing with one another the salts of the same acid, it has seemed to me more natural to refer their properties to equivalent quantities, or to quantities containing always the same proportion of acid. But if this has led me to group together two molecules of an alkaline chloride or nitrate, I was obliged, for the same reason, when taking the specific heats of sulphate of alumina or of alkaline phosphates to take, as unities of the weights of these salts, quantities which are really equivalent of other bodies, but which only represent fractions of the admitted equivalents of these bodies. Notwithstanding, M. Berthelot does not conclude that this is a proof that alumina should be written AlO and phosphoric acid $PhO\frac{5}{3}$, and that the equivalents of aluminium and phosphorus should be modified accordingly.

It may sometimes be interesting to compare certain properties of bodies by referring them to chemically equivalent weights, in cases where these correspond neither to molecular weights nor to the equivalents usually adopted, but we cannot conclude from this that those weights ought to be adopted as symbols of notations.

Apart from all these things, I agree with M. Berthelot that it is not advisable to exaggerate the importance of these questions, the solution of which cannot affect the important laws and theories of chemistry, and about which we can only reach a conclusion when we have arrived at more complete knowledge of the molecular constitution of compound bodies. This constitution itself will be doubtless revealed to us by researches on molecular mechanics, such as those on Thermo-chemistry, through which this eminent scientist aids so powerfully the advancement of science.

ART. XXIV.—*On some Reactions of Silver Chloride and Bromide;* by M. CAREY LEA, Philadelphia.

IN the course of some examinations made several years since on the reaction of silver bromide and iodide, I concluded that the dark substances produced by the action of light on these bodies are resolved by nitric acid into metallic silver which dissolves, and ordinary silver bromide and iodide, which remain. Since then I have met with a paper by Von Bibra on the reaction of silver chloride, in which that chemist found that darkened silver chloride was not attacked by nitric acid. My previous examination had not included the chloride, and the remarkable difference thus indicated, led me to return to the subject. I found my own previous results entirely confirmed as also Von Bibra's:—at least so far that only by the long continued action of nitric acid was there any decomposition of the darkened chloride, and even then, traces only of silver were taken up by the acid.

It therefore appears that the substance produced by the action of light on silver chloride is of a much more permanent character than in the case of the other silver haloids. Some other reactions noted in the course of the examinations which appear not devoid of interest, are given below.

Silver Chloride.

Our knowledge of the nature of the substances produced by the action of light on the silver haloids is very limited, owing to the fact that although the coloration is very intense, yet the portion of material acted upon is very small. Also because we have no means of removing from the mixture the unaltered haloid, except by agents that at the same time attack the altered substance, and completely change its character.

As to the first point—the small proportion of material actually altered by light. It is generally thought that silver chloride is reduced by the action of light to a sub-chloride containing half

as much chlorine as the normal white chloride. Yet the loss of chlorine has been found too small to be weighed. Fresenius doubts if a loss in weight could be detected by the most delicate balance, and Von Bibra in the investigation above referred to, could not find the slightest loss in weight.

With a view to obtain some quantitative indication in the matter, the following determination was made.

Silver chloride was precipitated with an excess of hydrochloric acid, was well washed, and exposed to bright sunlight for five days. During this time it was spread in a very thin layer over the bottom of a large white porcelain basin, was frequently stirred up to bring constantly new surfaces to the light, and was kept moistened with water.

Of the resulting dark powder two grams were taken and were thoroughly treated with sodium hyposulphite to remove the unaltered chloride. Previous experience had shown that extraordinary precautions were necessary to effect this thoroughly, as the removal of the last portions of normal silver chloride is very difficult. Accordingly the strong solution of hyposulphite was many times renewed, each time being left to act for from twelve to twenty-four hours. Finally the gray residue (metallic silver) was washed, dried and weighed, and found to amount to twenty-one milligrams.

It thus appears that as the result of five days' action of strong sunshine, with frequent stirring up and mixing to bring fresh portions to the light, about one per cent only of the silver chloride was acted upon. And if we suppose this action to consist in removing one-half the chlorine, then the whole loss in weight by the action of the light should be but little over one-tenth of one per cent. This proportion is of course not inappreciable, and the observations of Fresenius and of V. Bibra above quoted must be taken as referring to shorter exposures.

It was mentioned that another difficulty in verifying the nature of the action of light lay in the fact that those substances which can be used for dissolving out the unaltered chloride, also unfortunately attack the altered substance.

The two reagents most effectual for this removal are sodium hyposulphite, and liquid ammonia. That they also alter the blackened portion is at once evident from the fact that the residue which they leave behind instantly dissolves in cold nitric acid of sp. gr. 1·28 with evolution of red fumes, and is in fact, well known to be metallic silver, whereas before the application of these reagents, cold nitric acid has no effect, and even with boiling acid, applied for a long period only very slight decomposition ensues.*

* The facts here mentioned lead to the curious reflection that the permanency of ordinary photographic prints is greatly diminished by the fixing process. For in this fixing process which consists in immersion into solution of sodium hyposul-

When cold nitric acid, sp. gr. 1·28, is poured over a quantity of the darkened chloride (precipitated with excess of HCl) and left to remain for a time in contact, this acid acquires no silver reaction whatever. It is therefore certain that by the action of light on silver chloride (precipitated in presence of excess HCl) *no metallic silver is reduced.* And as this black substance is quickly whitened by aqua regia, it is reasonable to conclude that the dark matter contain less chlorine than the normal, and is either a subchloride or an oxychloride. Beyond this, we know nothing with certainty.

When the dark substance was boiled for several minutes with the same nitric acid, no silver was extracted. But when the vessel was placed on a sand bath and kept at or near boiling point for eighteen hours, renewing the acid as it escaped, a distinct effect was produced. The substance became a little lighter in color, and the acid was found to have taken up enough silver to show a strong opalescence by addition of hydrochloric acid; not enough however to give an immediate precipitate.

Ammonia and sodium hyposulphite have this in common, that both leave metallic silver behind when the darkened chloride is submitted to their action. In the case of the sodium salt, it is of course understood that it is presented in strong solution and very large excess.

Silver Bromide.

Silver bromide was precipitated with excess of KBr and well washed, and exposed to light.

When cold nitric acid, sp. gr. 1·28, was allowed to stand for one minute over the darkened bromide it took up silver abundantly. Allowed to act for an hour at a heat considerably below 212° the color of the darkened bromide had considerably changed and at the end of seven or eight hours, complete decomposition had taken place. The resulting AgBr is lemon yellow and has more the general appearance of iodide than of bromide.

Philadelphia, Jan., 1878.

phite, an exceedingly stable substance which almost completely resists the action of boiling nitric acid is reduced to metallic silver, in the condition of a thin layer of almost infinitesimally fine particles, and in that condition not well able to resist external influences. To a considerable extent this difficulty is obviated by a partial substitution of gold for the metallic silver.

The use of a fixing treatment can of course never be dispensed with, as it is essential to remove the unaltered chloride. But it is evident that if a substance could be found which would remove the unaltered chloride only, without attacking that which has been darkened by light, a great advantage would probably be gained The gold treatment could be dispensed with, the printing could be stopped when the right strength was obtained, without being carried further to allow for the weakening effect of the fixing agent, and the print obtained would probably be always perfectly permanent.

The fact that both sodium hyposulphite and ammonia in removing the unaltered chloride, reduce the altered to metallic silver, explains why the gold toning operation succeeds much better when applied after the fixing operation, than before it.

ART. XXV.—*On Journal Friction at Low Speeds;* by A. S KIMBALL, Professor of Physics in the Worcester Free Institute.

IN the Proceedings of the Royal Society, March 27, 1877, is published a paper by Professor Fleming Jenkin, on "Friction between Surfaces moving at Low Speeds," in which he establishes, by a series of very delicate experiments, conclusions at which the writer had previously arrived by a somewhat different course [see this Journal, March, 1876]. Professor Jenkin remarks, that in one case examined near the lower limit of velocity, "there was a slight and somewhat uncertain change of the opposite character, that is, a decrease of friction as the velocity decreased," and that "this case however, would require further examination." This phenomenon had been noticed in our laboratory, and an article had been written [see this Journal, May, 1877] embodying the results of our observations.

The effect of changes in velocity upon the coëfficient of friction is not an insignificant quantity, but has a sufficient magnitude to give it practical importance, in some cases at least. It therefore gives the writer great pleasure to be able to quote the experiments of so able an investigator, as confirming his conclusions, which are briefly these: The coëfficient of friction, at very low velocities, is small, it increases rapidly at first, then more gradually as the velocity increases, until at a certain rate, which depends upon the nature of the surfaces in contact and the intensity of the pressure, a maximum coëfficient, is reached. As the velocity increases beyond this point, the coëfficient decreases.

Later experiments have shown, what in fact might have been inferred, that from its maximum, the coëfficient decreases toward a constant value. The apparatus used in this series of experiments consisted of—1st. A pair of cone pulleys giving six changes of speed, driven by a shaft from the work shop. 2d. A rope transmission dynamometer, capable of indicating changes equal to one per cent of the largest, or five per cent of the smallest power transmitted. 3d. An experimental shaft with a pulley by which it was driven. The journals on this shaft were seven-eighths of an inch in diameter; they revolved in cast iron boxes three and one-half inches long, truly bored and polished. A thin cut was planed from the upper half of each box, so that when resting upon its journals it was not quite in contact with the lower half. Above each box was a lever of the second class, by means of which it was possible to apply any desired pressure to the revolving journals. The

course of the experiment is obvious. The experimental shaft, loaded with a known weight, was driven at different speeds, the required power noted, and the coefficient of friction calculated.

The results of seven series of experiments are shown in the following table, in which the first column shows the velocity of the circumference of the journal in feet per minute, the seven following columns giving the corresponding coefficients of friction. Since it was found impossible to maintain the same state of lubrication for any great length of time, every series which could not be completed in one half day was left unfinished.

TABLE I.

V.	*a.*	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*
·59′	·186	·180	·187	·174	·170	·127	·138
2·17	·153	·150	·148	·131	·129	·114	·109
5·15	·122	·128	·122	·080	·080	·097	·083
10 99		·089	·086	·069	·067	·080	·060
19·71			·068	·058	·066	·052	·053
42·86			·068		·060	·045	·041

In spite of the irregularities to be seen in this table, the law as stated above is clearly shown. Some of the discrepancies are doubtless due to variations in the state of lubrication of the journals. The pressure on the lower boxes in series *a*, *c*, *d* and *e*, was 130 pounds; in series *b*, *f* and *g*, 210 pounds. In *a*, *b*, *c*, *d* and *e*, the journals were lubricated by wiping them with a handful of waste saturated with sperm oil. In *f* and *g*, as much oil was poured through the oil holes as the boxes would hold. To assist the reader in forming an idea of the value of the average results in Table I, Table II is given, which contains the separate readings from which series *c* was computed. Eight readings were made at each speed. The readings are the differences in tension of the two parts of the belt driving the experimental shaft:

TABLE II.

Speeds,	·59′	2·17′	5·15′	10·99′	19·71′	42·86′
Differences in lbs.	5·5	4·3	3·5	2·5	2·0	2·0
	5·4	4·2	3·6	2·6	1·9	1·9
	5·3	4·3	3·6	2·5	1·9	1·9
	5·4	4·4	3·5	2·5	2·1	1·9
	5·4	4·2	3·5	2·4	1·9	2·0
	5·5	4·3	3·5	2·3	2·0	2·0
	5·4	4·3	3·6	2·5	2·0	2·0
	5·4	4·3	3·6	2·5	1·9	1·9
Averages,	5·41	4·28	3·55	2·47	1·96	1·96
Coefficients,	·187	·148	·122	·086	·068	·068

The variation in observations taken at the same speeds, is nearly identical in every series with that shown in Table II.

In order to eliminate as far as possible gradual changes in the condition of the rubbing surfaces, the following order of observation was adopted in each series. Calling the slowest speed No. 1, four observations were made alternating

	between	Nos.	6 and	1.
Four	"	1	"	2.
"	"	2	"	3, etc.
"	"	5	"	6.
"	"	6	"	5, etc.
"	"	2	"	1.
"	"	1	"	6.

About 1,000 separate experiments were made under conditions of lubrication, and pressure upon the journals, purposely varied, so that the average result should represent as nearly as possible ordinary shop practice. Speeds as high as 100 feet (or 440 revolutions of the experimental shaft) per minute were employed. The results though not so uniform as those at lower speeds were decisive. The most probable values of the coefficient as shown by a graphical construction of all these experiments is shown in

TABLE III.

Speed,	1′	3′	5′	7′	10′	15′	20′	30′	40′	60′	80′	100′
Coefficient,	·150	·122	·104	·093	·079	·066	·058	·054	·053	·052	·051	·050

Thus we see that the conditions under which journal friction usually occurs are such that the maximum coefficient will be found at a very low speed. While using a journal 6″ in diameter with a load of only ten lbs., a reduction was made in the speed till the maximum coefficient (in this case ·27) was passed, after which the coefficient diminished very quickly to one-third of its maximum value. In the case of leather sliding on iron or wood where the intensity of the pressure is not great the coefficient will usually increase with the velocity. Examples of this will be found in the leather packings of hydraulic engines and in the slipping of belts upon pulleys.

It is usually assumed that the coefficient of friction is constant for all weights, not so light that the influence of adhesion is great in comparison, or on the other hand so great as to produce abrasion. An experimental examination of this point gave the results recorded in Table IV. Each coefficient is a mean derived from several observations, and though the results of experiments with constant weights were very uniform, it was found that the weights could not be changed without disturbing slightly the adjustment of the machine. These results seem to indicate that the coefficient of friction decreases slightly as the specific pressure on the journal increases even when the pressures are quite large.

TABLE IV.

Pressures are given in pounds on a square inch of longitudinal journal section.

P.	Coef'ts at ·59′ per min.		Coef'ts at 2·17′ per min.	
23·5	·154	·159	·136	·130
36·8	·157	·156	·138	·130
50·1	·149	·147	·119	·124
63·4	·149	·153	·117	·127
76·7	·153	·152	·108	·126
90·0	·151	·150	·109	·123
103·3	·155	·150	·111	·127
116·6	·149	·145	·113	·124
129·9	·147	·146	·112	·123
143·2	·145	·146	·113	·121
156·5	·147	·147	·113	·124
169·8	·146	·146	·112	·121
183·1	·145	·148	·109	·124
196·4	·144	·145	·111	·125

The larger part of the experiments referred to were made by Mr. Desper, a student of the Institute, whose patience and care deserve especial mention.

ART. XXVI.—*Observations of the Brightness of the Satellites of Uranus.*

[Communicated by Rear-Admiral John Rodgers, U.S.N., Superintendent U. S. Naval Observatory.]

THE following notes on the brightness of these bodies have been made during the regular series of measures for position. They are printed in order that they may be of service to any astronomer who may undertake an exact photometric determination of their brightness. The observations were made with the xxvi-inch telescope with powers of 600–900 diameters. The observers were H (Hall) and HN (Holden).

It was surmised by Professor Newcomb that the brightness of *Ariel* varied in different parts of its orbit (Wash. Ast. Obs. 1874, Appendix I, p. 43,) and that it was least bright in $p=180°\pm$ during 1874.

There have been observed in 1874–5–6–7 nineteen position angles of *Ariel:* of these, seven were $180°\pm$, and twelve were $0°\pm$.

A better test is the following: on fifteen nights this satellite was bright enough to allow of measures of distance. On six of these fifteen nights the position was $180°\pm$, and on nine the position was $0°\pm$. It would seem, therefore, to be more easily observed in one half of its orbit than in the other, so far as this evidence goes.

Dr. H. C. Vogel has suggested that *Titania* also is of varying brightness in different parts of the orbit. Early attention was given to this point. The following extracts from the observing books may be of use, when an exact photometric determination comes to be made. The weight indicates the steadiness of the images, five being perfectly steady. The positions given are to the nearest degree in p and nearest second in s.

1875. Jan. 26. *Titania*; $p=260°$, $s=19''$. Wt. 4. Titania is fainter than I have ever seen it.—HN.

1875. March 4. Four stars visible besides *Oberon* and *Titania*.

Oberon,	$p=274°$	$s=26''$
Titania,	$p=184°$	$s=35''$
Star 1,	$p=350°\pm$	$s=80''\pm$
Star 2,	$p=20°\pm$	$s=100''\pm$
Star 3,	$p=130°\pm$	$s=130''\pm$
Star 4,	$p=95°\pm$	$s=200''\pm$

Order of brightness; 4, 1, 3, 2; and 2 somewhat brighter than Titania.—HN.

1875. May 25. *Oberon*: $p=230°$, $s=31''$.
Titania: $p=21°$, $s=30''$.

Oberon very faint [and therefore as nothing is said of brightness of *Titania*], *Titania* brighter than *Oberon* at same distance.—H.

1876. Jan. 14. *Oberon*: $p=11°$, $s=23''$ } Wt=2.
Titania: $p=48°$, $s=27''$ }

Titania much brighter than *Oberon*. Moonlight.—HN.

1876. Jan. 20. *Oberon*: $p=321°$, $s=27''$ } Wt=2.
Titania: $p=186°$, $s=34''$ }

Measures of *Oberon* quite difficult [and of *Titania* not so.]—HN.

1876. Jan. 25. *Oberon*: $p=181°$, $s=44''$ } Wt=2.
Titania: $p=352°$, $s=31''$ }

Oberon easier to see [brighter] than *Titania*.—HN.

1876. Jan. 26. *Titania* in $p=310°$, $s=16''$. Wt. 5.

This is about the smallest distance at which *Titania* has been measured.

1876. Jan. 31. *Titania*: $p=76°$, $s=17''$ } Wt=5.
Ariel: $p=29°$, $s=12''$ }

Titania is at least twice as bright as *Ariel*. *Titania* is decidedly brighter than the satellite of Neptune. It is easier to see (under these conditions, Wt=5) within 16″ of Uranus, than the satellite of Neptune within 16″ of Neptune.—HN.

1876. Feb. 2. *Oberon*: $p=338°$, $s=33''$ } Wt=4.
Titania: $p=7°$, $s=34''$ }

After careful examination with 800 A and 400 A, I cannot decide which is brighter, *Oberon* or *Titania*, but if there is any difference, *Titania* is the brighter.—HN.

1876. Feb. 18. *Oberon:* $p=228°, s=30''$ } Wt=4.
Titania: $p=44°, s=24''$ }

Titania brighter than *Oberon* but not very much so.—HN.

1876. Feb. 20. Star near *Uranus;* $p=89°$, $s=27''$. Wt=1.

This star is about the brightness of *Oberon* and *Titania.* [For *Oberon* $p=276°\pm$; for *Titania* $p=131°\pm$].—HN.

1876. March 3. Two stars near Uranus, both slightly brighter than *Oberon.* Star 1, $p=230°$, $s=60''$ } estimated.
Star 2. $p=0$, $s=50''$ }

Not much difference in the brightness of *Oberon* and *Titania.* [For *Oberon* $p=236°\pm$, for *Titania* $p=268°\pm$].

Umbriel: $p=23°$, $s=20''$ } Wt=3.
Ariel: $p=173°$, $s=13''$ }

Umbriel is more steadily seen than *Ariel* but not much more so.

Then reduced aperture to fifteen inches. *Umbriel* was certainly seen and its position could have been measured. *Ariel* was not certainly seen. Sky hazy. Bright moonlight.—HN.

1876. Mar. 4. *Ariel:* $p=11°$, $s=13''$. Moonlight and haze.

Ariel is quite as bright as last night in $p=180°\pm$ under better circumstances.

Oberon: $p=198°$, $s=44''$ } Wt=4.
Titania: $p=177°$, $s=32''$ }

Titania is brighter than *Oberon* although not much so.—HN.

1876. March 9. *Oberon:* $p=57°$, $s=27''$ } Wt=3.
Titania: $p=340°$, $s=26''$ }

Titania is decidedly brighter than *Oberon.*—H.

1876. March 13. *Oberon:* $p=347°$, $s=38''$ } Wt=2.
Titania: $p=170°$, $s=30''$ }

Oberon is brighter than *Titania.*—H.

1876. March 14. *Ariel:* $p=23°$, $s=14''$
Umbriel: $p=176°$, $s=19''$
Titania: $p=126°$, $s=18''$ } Wt=2.
Oberon: $p=318°$, $s=27''$

Ariel a little brighter than *Umbriel. Titania* very faint. *Oberon* brighter than *Titania.*—H.

1876. March 22. *Oberon:* $p=78°$, $s=23''$ } Wt=3.
Titania: $p=161°$, $s=26''$ }

Titania much brighter than *Oberon.*—H.

1876. March 23. *Oberon:* $p=42°$, $s=33''$ } Wt=3.
Titania: $p=106°$, $s=16''$ }

Oberon brighter than *Titania.*—H.

1876. March 31 *Oberon:* $p=198°$, $s=43''$ } Wt=2.
Titania: $p=152°$, $s=24''$ }

Oberon brighter than *Titania.*—H.

It is not desirable to draw any conclusions from these observations as yet. They are here recorded as likely to be of service in photometric observations, and they will be continued as occasion offers.

ART. XXVII.—*A new method for the decomposition of Chromic Iron;* by EDGAR F. SMITH.

RECENTLY I was led to try the action of bromine and sodium hydrate upon pulverized chromic iron, and as the amount of chromium extracted in this manner was rather surprising, the following experiments were made, to ascertain what effect bromine alone would have upon the same substance.

I. Moderately fine chromic iron (·1500 grams) was placed in a tube of hard glass and after adding dilute bromine water and sealing the tube, the latter was placed in an air-bath and heated for twelve hours at a temperature about 130° C. When cool the tube was opened and its contents poured upon a filter. The insoluble residue was thoroughly washed by decantation, and upon the filter, with hot water. The filtrate after concentration was treated with a slight excess of ammonium hydrate, causing the precipitation of aluminum hydrate, etc. The latter was filtered off and the yellow-colored filtrate then warmed with hydrogen sulphide to reduce the chromic acid to oxide. The precipitate formed, after protracted digestion, was allowed to settle and the clear liquid filtered. After washing, the precipitate was dissolved in a few drops of dilute hydrochloric acid and re-precipitated. This operation was repeated and the precipitate finally transferred to a filter, washed, dried and ignited. The amount of chromic oxide found corresponded to 15·50 per cent of the substance taken.

The amount of chromium still remaining in the unattacked material was not estimated.

II. ·2000 grams substance, as finely pulverized as could be obtained by grinding the material in an agate mortar, were heated in a sealed tube with water saturated with bromine and a few drops of bromine. The tube remained in the oven four days, the temperature ranging from 175°–190° C. Upon opening the tube its contents were poured into a beaker and evaporated; water added and the solution filtered. The residual, unattacked mineral powder after washing, drying and igniting, weighed ·0820 grams. The filtrate from this was treated precisely as in (I) and the chromic oxide obtained from it amounted to 28·05 per cent.

III. In this experiment only ·1500 grams substance were employed. The material was of the same fineness as in (II). Instead of using dilute bromine water as heretofore an excess of bromine was poured over the substance and but a very small quantity of water added. For three days the tube was exposed to a temperature varying from 150°–175° C. At the expiration of this time the tube was examined, and as the sub-

stance appeared to be perfectly decomposed, the solution was removed from the tube and evaporated in a beaker to expel the large excess of bromine, upon the gradual disappearance of which a dark powder showed itself. The solution was strongly diluted with water and filtered. The insoluble residue was thoroughly washed with hot water. Dried and ignited, this weighed ·0140 grams.

The filtrate was mixed with an excess of ammonium hydrate and evaporated almost to dryness in a casserole. The solution was then diluted with water and filtered from the aluminum hydrate, etc., and treated as in (I). The percentage of chromic oxide extracted equaled 49·60 per cent.

IV. From the preceding experiments it appeared very evident, that all that was lacking to render the decomposition complete was to have the chromic iron in an exceedingly fine condition. To this end the material that had been ground to an impalpable powder in an agate mortar was elutriated, then dried and two distinct portions of ·1500 grams each placed in good hard glass tubes. To each portion was added a rather large quantity of bromine water and from ten to twelve drops of bromine. Both tubes were heated for one day at 130° C. For two successive days the temperature was maintained at 170° C. At the expiration of the third day, one of the tubes was removed from the oven and opened. Red oxide of iron had separated and undecomposed material was no longer visible. The whole was poured into a beaker and evaporated, water added and filtered. The residue was thoroughly washed, dried and ignited, then transferred to a beaker and heated with dilute hydrochloric acid. The entire mass dissolved readily and without a residue. The decomposition was, therefore, complete.

The filtrate from the iron oxide was evaporated almost to dryness after the addition of an excess of ammonium hydrate, then diluted and filtered. The solution was reduced with hydrogen sulphide and the precipitate, after filtering and washing, dissolved in dilute hydrochloric acid and re-precipitated with ammonium hydrate. This operation was repeated and the chromium oxide obtained was 62·66 per cent.

The second tube, removed not long after the first, contained a large amount of separated ferric oxide. This after filtering off the chromium solution also dissolved very readily in warm, dilute hydrochloric acid, leaving not the least trace of residue. The filtrate from this, after being similarly treated as above, yielded 62·83 chromium oxide.

These results accord with those of Garrett, who analyzed the same ore from Texas, Pa., and obtained about 63 per cent chromic oxide.

The ferric oxide that separates out in the tube during the decomposition will not contain any chromium, if it is thoroughly washed with hot water. In no instance was iron found in the solution containing the chromic acid.

Several tubes containing the pulverized substance, potassium hydrate and bromine water were heated at 125° C., but invariably exploded before the decomposition was completed, and therefore no farther attempts were made to use the alkali to aid in the decomposition.

All that is necessary to effect the complete decomposition of chromic iron by this method is that the substance be exceedingly fine and that the same be exposed with bromine to a temperature of 180° C. from two to three days. The addition of ten to twelve drops bromine hastens the decomposition very decidedly.

In connection with the above it may be well to mention that the insoluble chromic oxide obtained by the ignition of the corresponding hydrate, may be brought into solution again by digesting it with bromine and sodium hydrate in a beaker.

Laboratory of the University of Pennsylvania, Dec. 22, 1877.

ART. XXVIII.—*Descriptions of two new genera of Pycnogonida;* by E. B. WILSON. *Brief Contributions to Zoology from the Museum of Yale College.* No. XXXVII.

Anoplodactylus, (nov. gen.)

Body slender. Rostrum cylindrical, rounded. Antennæ three-jointed, chelate. Palpi wanting. Ovigerous legs six-jointed, wanting in the male. Neck elongated, extending forward over the rostrum. Legs nine-jointed. Dactylus without auxiliary claws.

THIS genus differs from *Phoxichilidium*, which it otherwise closely resembles, in the number of articulations composing the ovigerous legs, and in the absence of auxiliary claws upon the dactylus. These claws, though comparatively small in *Phoxichilidium*, are very large in certain genera (e. g. *Pallene* Johnst.) They are movably articulated to the dactylus and furnished with a special set of muscles by means of which they are moved.

To this genus should be referred Kröyer's *Phoxichilidium petiolatum* (Voy. en Scand., Laponie, etc., Pl. 38, fig. 3) and probably also the *Phoxichilidium virescens* of Hodge.

Anoplodactylus lentus (sp. nov).

Phoxichilidium maxillare Smith, in Rep. on the Invertebrates of Vineyard Sound, p. 250, Pl. VII, fig. 35, 1874 (*non* Stimpson).

Oculiferous segment broad, as long as the two following segments taken together, not emarginate between the bases of the antennæ. Neck swollen. Posterior segment of body very slender. *Abdomen* rather more than twice as long as broad, slightly bifid at the extremity.

Oculiferous tubercle prominent, acute, placed on the anterior portion of the first segment. Eyes four, ovate, varying in color from light brown to black.

Rostrum very large, longer than the oculiferous segment, constricted at base, thus appearing somewhat clavate. The extremity is subglobose.

Antennæ hairy, long and slender, their bases closely approximated. Basal joint extending beyond extremity of rostrum. Chela stout, hairy. Dactylus very stout, smooth on margin.

Ovigerous legs stout, roughened by minute tubercles, the outer joints with many strong hairs, most of which are directed backward. The two basal joints are very thick; the first is shorter than its width, the second about twice the first. The succeeding joints are much more slender. The third is nearly two and a half the second, somewhat clavate, and suddenly constricted a short distance from the base. Fourth joint half the third. Fifth considerably less than fourth. Terminal joint much smaller than the preceding.

Fig. 1.—*Anoplodactylus lentus.* *a*, Terminal joints of leg; *b*, ovigerous leg.

Legs very long and slender. First and third joints very short, the second considerably longer and clavate. The three following joints are much longer, the sixth being the longest. The seventh or tarsal joint is very short and deeply emarginate exteriorly. Eighth joint (propodus) curved, with a rounded lobe near the base, which bears five or six strong spines. Exterior to these is a series of much smaller stout spines. The dactylus is stout, about two-thirds as long as the propodus. The whole surface of the body is scabrous. The legs bear a few scattered hairs, which are more numerous on the outer joints.

The genital orifices are situated on the lower side of the second joint of the legs, near the external margin. The sexes resemble each other closely except in the absence, in the male, of the ovigerous legs. The males are also, as a rule, slightly larger than the females. "It is most frequently deep purple in color, but gray and brown specimens are often met with." (*Verrill*).

This species is common in Vineyard Sound, but does not

occur northward. It resembles the "*Phoxichilidium*" *petiolatum* of Kröyer, which has, however, according to the figures, the anterior segment much more slender and emarginate between the bases of the antennæ which are thus separated by a distinct interval, the posterior segment is represented as stouter and shorter, the proboscis more abbreviated, and the propodous of a different shape. Kröyer figures the ovigerous legs with seven joints, probably mistaking the constriction near the base of the third joint for an articulation.

Length of body in largest specimens (inclusive of rostrum and abdomen), 7 millimeters. Legs, 30 millimeters.

Pseudopallene, (gen. nov.).

Body robust. Neck broad and thick. Rostrum more or less acute. Antennæ three-jointed, chelate. Palpi wanting. Ovigerous legs composed of eleven joints. Legs nine-jointed. Dactylus without auxiliary claws.

This genus has not hitherto been distinguished from *Pallene*, and this has led to some confusion in the diagnosis of that genus.

In *Pallene*, as described by Johnston (Mag. Zool. and Bot., vol. i, p. 380), the ovigerous legs are nine-jointed, the neck is constricted and more or less elongated as in *Nymphon*, the rostrum is very short and nearly hemispherical, and the dactylus is armed with two very large auxiliary claws. A *Pallene*, to be hereafter described, collected by Prof. S. I. Smith at Wood's Holl, Mass., agrees in all these points with Johnston's *P. brevirostris*. The three species: *Pallene spinipes*, *P. intermedia* and *P. discoidea* figured in Gaimard's Voy. en Scand., Laponie, etc., do not belong to the *Pallene* of Johnston, but should probably be referred to *Pseudopallene*. Having seen no specimens of these species, I have been unable to verify this.

Pseudopallene hispida.

Pallene hispida Stimpson, Inv. of Grand Menan, p. 37, 1853.

Of this species there is but a single female specimen in the collection of the Peabody Museum, dredged by the United States Fish Commission in Johnson's Bay, near Eastport, Maine, in 12 fathoms rocky bottom. Stimpson records it from deep water off Grand Menan, "on *Ascidiæ callosæ*." His description being incomplete in several particulars, a full description is here given.

Body very broad, oval, neck not constricted. Oculiferous tubercle small, rounded. Eyes four, ovate, light brown. Oculiferous segment half as long as the body. Second and third segments with three prominent tubercles above, each of which is tipped by a hair. The lateral thoracic processes are very broad and are not separated by any interval; they bear on the outer margin two to four spine-like, hairy tubercles. Abdomen twice as long as broad, truncate, hairy.

Rostrum as long as oculiferous segment, with a constriction on each side below, giving it the appearance of being articulated at this point, acute-conical, with a rosette of filamentary processes around the terminal mouth.

Antennæ hairy, stout and swollen, about twice as long as the rostrum, tipped with amber color. Basal joints enlarged near their attachment. The second joint has a prominent rounded tubercle on the lower end, behind which the dactylus closes.

Ovigerous legs slender, eleven-jointed, terminal joint claw-like, trifid. Fifth joint somewhat clavate, considerably smaller than the fourth. The four outer joints are armed with three or four stout, smooth, curved spines.

Fig. 2.—*Pseudopallene hispida.* *a*, Terminal joints of leg; *b*, ovigerous leg; *c*, terminal joint.

Legs very stout, the three basal joints short, overlapping each other in an imbricated manner. Fourth joint as long as the three basal joints taken together, much distended by the ovaries in the specimen described. Fifth, as long as the fourth but much more slender. Sixth, longer and more slender. Seventh (tarsus) very short, nearly triangular. Eighth slightly curved, armed with five or six spines on the inner (concave) margin. Dactylus slender, curved, acute, without accessory claws, about two-thirds as long as the preceding joint.

All of the legs bear more or fewer prominent, conical, spiny tubercles. These are arranged in longitudinal rows on some of the joints, particularly on the fifth and sixth, which appear deeply serrate on the external margin. The entire surface of the body is rough, and more or less hairy.

Genital orifices small, on the second joint of the legs.

Length (inclusive of rostrum and abdomen) 3 millimeters. Legs, 7·5 millimeters. Ovigerous legs, 3·7 millimeters.

ART. XXIX.—*Tantalite from Coosa County, Alabama, its mode of occurrence and composition;* by J. LAWRENCE SMITH, Louisville, Ky.

WHILE columbite has been long known from a number of localities in the United States and at some of them it is found in great abundance, the related mineral tantalite has never been identified, until recently I proved the fact of its occurrence in Alabama. Professor König has described (Proc. Acad.

Nat. Sci. Philadelphia, 1876) a mineral which he considered to be tantalite from Yancey County, North Carolina. There must however have been some mistake about the matter, for he states the specific gravity of the mineral to be 5·807. If this determination was given correctly this could not have been tantalite. I have found columbite from the North Carolina localities with specific gravities from 5·6 to 6·3, varying according to the amount of tantalic acid present with the columbic acid. There is no instance that I know of where tantalite has as low a specific gravity as 7.

I am indebted to Professor Eugene Smith, State Geologist of Alabama, for the specimen of tantalite that first came under my observation; he suspected it to be tantalite and sent it to me for verification; he had obtained the specimen from Judge Bently, to whom we owe what we know about the manner of its occurrence.

It is found in Coosa County, Alabama, detached from any rock, lying loose with "bowlders" (as Judge Bentley calls them) of granite more or less disintegrated. As, however, this region belongs to the older series of rocks (Professor Eugene Smith has not yet explored it) these blocks of granite are doubtless not bowlders, but detached masses, weather worn. They are found both under and on the surface for miles, running northeast and southwest. Across these in a direction northwest and southeast runs a ridge filled with quartz and flint rocks and at the intersection of the two, over about an acre of surface, some fifty specimens of tantalite have been collected, from the size of a pea to a lump one and a quarter pounds in weight.

Character of specimens.—They are irregular masses, without the slightest indication of crystalline form, just such pieces as I have obtained from the locality at Limoges; they are more or less rounded, with a ready cleavage in one direction; the specimens although long exposed, have undergone but little alteration, as indicated by examination made on several specimens. The specific gravity varied from 7·305 to 7·401.

On analysis it was found to consist of:

Tantalic acid,	79·65	81·62 metalic acids.
Tungstic "	1·10	
Stannic "	·87	
Manganese protoxide,	3·72	
Iron "	13·51	
Copper oxide,	·89	
	99·74	

The tantalic acid contains very little columbic acid.

Judging from the discoveries already made, and the large size of some of the pieces of tantalite, we may expect some important results from the future explorations in Alabama.

ART. XXX.—*On Amylidenamine Silver Nitrate;* by W. G. MIXTER. *Contributions from the Sheffield Laboratory of Yale College,* No. XXX.

STRECKER (Liebig's Ann., cxxx, p. 220) states that silver nitrate produces in alcoholic solutions of valeralammonia, a white precipitate, which slowly turns black in the cold, but he says nothing as to the composition of the substance. I have obtained what is doubtless the same compound by the spontaneous evaporation of alcoholic ammonia solutions of valeralammonia to which an excess of silver nitrate had been added. The valeral was made from rectified commercial fusel oil by Parkinson's process. It did not give a constant boiling point. Different portions of it were treated with dilute ammonia water and the crystalline valeralammonia was washed on the pump with water. The product used for the first series of analyses was made by dissolving ten grams silver nitrate in 100 c. c. strong ammonia water and an equal volume of alcohol and then adding thirty grams of hydrous valeralammonia, from valeral boiling 95°–105°. Considerable oil separated which was dissolved by adding alcohol. The solution was exposed in a wide dish to the air until crystals ceased to form. The crystalline product was washed first with alcohol then with water, with alcohol again and finally with ether. It weighed, after drying over oil of vitriol, about six grams. The material for the second series of analyses was from thirty-four grams hydrous valeralammonia dissolved in 175 c. c. alcohol and 9·5 grams silver nitrate previously dissolved in 25 c. c. concentrated ammonia water. The crystals from this solution were washed as above described. The product weighed 12·5 grams. The filtrates in both experiments gave no more crystals until ammonio nitrate of silver separated, but gave a dark oil and some reduced silver. The carbon and hydrogen were determined by means of lead chromate, the nitrogen by Schiff's method and the silver by ignition.

	1st Series.	2d Series.	Calculated for $C_{15}H_{33}N_4O_3Ag$.
Carbon	42·11	42·26	42·35
Hydrogen	7·89	7·90	7·76
Nitrogen	13·24	13·18	13·18
Silver	25·26	25·19	25·41
Oxygen	[11·50]	[11·47]	11·30
	100·00	100·00	100·00

The equation $3(C_5H_{11}ONH_2)+AgNO_3=C_{15}H_{33}N_4O_3Ag+3H_2O$ doubtless represents the formation of the substance in question. It is not possible to give a gravimetric proof on

account of some decomposition which occurs during the evaporation. The compound dissolves with but partial separation of silver in boiling water. The clear aqueous solution evaporated on a water bath leaves a slight black residue and a very soluble crystalline mass, which contains silver, reacts for ammonia and gives red fumes when heated with oil of vitriol. By distilling with ammonia water crystals are obtained in the distillate which reduce silver. Hot dilute acids decompose it with the separation of an oil which has the odor of valeral, and hot oil of vitriol evolves nitrous fumes from it. The reactions show that the substance contains the amyliden, ammonio and nitro groups and that it is an amine analogous to Rose's $3NH_3AgNO_3$, thus:

$$\left.\begin{matrix} H_3N \\ H_3N \\ H_3N \end{matrix}\right\} AgNO_3 \qquad \left.\begin{matrix} C_5H_{10}{=}NH \\ C_5H_{10}{=}NH \\ C_5H_{10}{=}NH \end{matrix}\right\} AgNO_3$$

The name amylidenamine silver nitrate is perhaps the best that can be given to the substance until more is known of its constitution. If the corresponding ammonio compound be regarded as diammonium-argentammonium nitrate (Graham–Otto, iii, 840) the derivative from valeralammonia may be regarded as di-amylidenammonium-argentamylidenammonium nitrate, thus:

$$\begin{matrix} Ag \\ | \\ H_2N{-}O{-}NO_2 \\ | \\ H_3N \\ | \\ H_4N \end{matrix} \qquad \begin{matrix} Ag \\ | \\ C_5H_{10}{=}N{-}O{-}NO_2 \\ | \\ C_5H_{10}{=}NH \\ | \\ C_5H_{10}{=}NH_2 \end{matrix}$$

Amylidenamine silver nitrate is insoluble in water, ammonia water, alcohol and ether, but freely soluble in ammoniacal alcohol from which it crystallizes. The crystals were too small and poorly defined for measurement. Hydrates do not appear to have formed. Dilute hydrochloric acid decomposes the body and the filtrate from the silver chloride formed yields on evaporation ammonium chloride. Hydrogen sulphide decomposes the amylidenamine silver nitrate when suspended in ether, with the formation of a black mass which contains sulphur, silver, a hydrocarbon or undecomposed substance and ammonium nitrate. The etherial solution after washing with water was evaporated and the thick yellowish oil remaining was dried over oil of vitriol. The following is the analysis of it:

		Calculated for.	
Carbon	64·99	C_{25}	65·79
Hydrogen	11·25	H_{50}	10·97
Nitrogen	9·53	N_3	9·21
Sulphur	14·01	S_2	14·03
	99·78		100.00

The results indicate a mixture of $C_{15}H_{30}N_2$ and $2(C_5H_{10}S)$. The oil has the odor of thiovaleral, and decomposes when distilled. Hydrochloric acid added to the concentrated etherial solution of it produces a white curdy mass which was not obtained free from adhering oil. This last product is soluble in alcohol, ether, and with the separation of an oil in hot water. Platinic chloride produces at once in the alcoholic solution a small light yellow precipitate and in a few minutes a dark brownish red curdy precipitate which yields to water, platinchloride of ammonium. The red precipitate after washing with water and alcohol, reacts for platinum, sulphur and a hydrocarbon. Lack of material prevented further investigation of the product from amylidenamine silver nitrate by the action of hydrogen sulphide.

New Haven, January 10, 1878.

ART. XXXI.—*Note on the Crystallization of Variscite;* by ALBERT H. CHESTER.

A MICROSCOPIC examination of certain small crystals of the mineral variscite from Arkansas reveals the following facts with reference to its method of occurrence and crystallization.

The crystals are rarely distinct, but are usually found in complicated groups, sometimes forming clusters of a sheaf form. Very rarely single prismatic crystals are found, sufficiently distinct to admit of measurement.

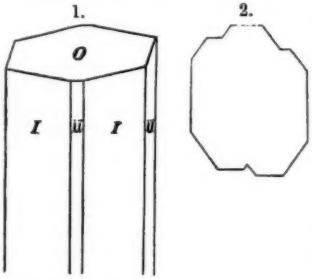


Fig. 1 gives the most common form, belonging to the orthorhombic system, and showing faces of *I*, *ĭĭ*, *i*̆*ĭ* and *O*. In this crystal $I \wedge I = 114° 6'$. In general but one termination is seen, but crystals showing both ends are sometimes found lying on the quartz matrix, the bases being similar to each other. The face *ĭĭ* is very small and therefore easily overlooked, and *i*̆*ĭ* is about the same size as *I*, so that these crystals may readily be mistaken for hexagonal prisms. Crystals showing a simple termination like that in fig. 1 are seldom seen. More frequently the basal plane is like fig. 2, or still more complicated. These planes are often covered with a thin opaque white coating, probably of quartz.

A striking peculiarity of this mineral is its high lustre, like that of beryl, which it much resembles when viewed under a low power. The crystal figured above is 0·3 mm. in diameter, and is about the average size of those examined.

Hamilton College Laboratory, January 19th, 1878.

ART. XXXII.—*Discovery of a New Planet;* by C. H. F. PETERS. (From a letter to one of the Editors.)

IN the night of February 3–4, in revising one of my Zodiacal Charts, I found a star that I could not have omitted, as being of the tenth magnitude. Some measures therefore were immediately taken, which showed that the object is a planet hitherto unknown. Its position was,

Feb. 3, $13^h\ 46^m\ 47^s$ m. t. $\alpha = 10^h\ 1^m\ 38^s{\cdot}08$. $\delta = +11^\circ\ 23'\ 34''{\cdot}1$,

from ten comparisons with Dm. $+11^\circ.2173$,—the place of this star being determined by differentiation from LL. 19882, of which there are several modern determinations, viz: W. $10^h.91$, R. 3090, Arg. $+11^\circ.2190$, Berlin Mer. Cir. in A. N., No. 1383.

Last night it clouded up, before the planet could be re-observed. But from the measurements of the preceding night follows the hourly motion $-1^s{\cdot}75$ and $+27''{\cdot}0$, or the daily motion -42^s and $+11'$, so that there will be no difficulty in finding the planet again, it having now already entered upon Chacornac's chart.

There has been some confusion of late in the numbering of planetoids, arisen from neglect of prompt communication. But it seems this new member will have to carry the number 180. If the priority of discovery remains to me, I propose the name *Eunike*, in commemoration of the glorious victories won by the Russian armies in their strife for humanity.

Litchfield Observatory of Hamilton College, Clinton, N. Y., Feb. 5, 1878.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *New mode of Determining Vapor Densities.*—NAUMANN has proposed an entirely new mode of determining vapor densities, founded upon the well established principle that when substances not miscible with a given liquid are distilled with this liquid, the quantity of the two bodies in the distillate, is, at a constant temperature of ebullition, in a constant ratio. Since, on the mechanical theory of gases, the vapor-tension, other things being equal, depends on the number of molecules which the vapor contains, the author sought a relation between the constituents of the distillate, expressed in molecular weights, and the vapor tensions of these constituents in the vapor mixture. The results confirm completely the hypothesis and establish the following law: The ratio of the quantities of the substances in the distillate, expressed in molecular weights, is equal to the ratio of the vapor-tensions of these constituents in the vapor mixture, meas-

ured at the temperature of ebullition. If g is the weight of one substance in the distillate, m its molecular weight and p its vapor tension at the boiling point t and under the barometric pressure b; and if G be the weight of the other constituent, M its molecular weight and P its vapor tension also at the boiling point, we have, by the above law,

$$\frac{\frac{g}{m}}{\frac{G}{M}} = \frac{p}{P}.$$

An extended series of experimental results are given, which establish the equality of these two ratios. From this equation it follows that $M = \frac{mGp}{gP}$. If one of the substances be water, $m = 18$, p is obtained from Regnault's table of tensions of aqueous vapor, g and G are the quantities of the two bodies found by analysis in the distillate, and $P = b - p$. With naphthalene for example, distilled in steam, 49·4 grams of water and 8·9 grams of naphthalene went over at 98·2°, under a barometric pressure of 733 mm. For $t = 98·2°$, the tension of aqueous vapor $p = 712·4$ mm. and hence the naphthalene vapor tension $P = b - p = 733 - 712·4 = 20·6$ mm. The molecular weight of water $m = 18$. Substituting these values, $M = \frac{18 \times 8·9 \times 712·4}{49·4 \times 20·6} = 113$. The formula $C_{10}H_8$ requires 128. This is an extreme case, but the result shows that the above formula, and not any multiple or submultiple of it, is correct. The method is capable of indefinite extension.—*Ber. Berl. Chem. Ges.*, x, 2098, Jan., 1878. G. F. B.

2. *On a new Oxide of Sulphur, Persulphuric oxide.*—Berthelot has described a new oxide of sulphur which he calls persulphuric oxide, corresponding to perchromic and permanganic oxides. It may be obtained by the action of the silent electric discharge, under a high tension, upon a mixture of equal volumes of absolutely dry sulphurous oxide and oxygen gases. In solution as persulphuric acid, it is produced in the electrolysis of concentrated sulphuric acid, and has been confounded with the so-called antozone. It is also formed when hydrogen peroxide is mixed with concentrated sulphuric acid. The oxide is best prepared with the concentric tube apparatus,* by the action of electricity as above. At the end of eight to ten hours, the walls of the annular portion are covered with drops of a thick adherent liquid. Exposed to a temperature of 0°, it crystallizes sometimes in granular indistinct crystals, sometimes in transparent thin and flexible needles, many centimeters long and of an appreciable breadth, often crossing the tube. It resembles sulphuric oxide, but the latter is opaque, in finer needles, shorter and narrower. It possesses a considerable vapor tension and sublimes spontaneously in the

* Ann. Chim. Phys., V, xii, 463, December, 1877.

tubes containing it. Its composition was determined by synthesis and by analysis. Synthetically, the residual gas after the action was withdrawn by a mercury pump and measured; it was one-eighth of the original gas, corresponding to the equation $(SO_3)_2+O_4=S_2O_7+O$; or in volumes 4+4 originally and 1 finally. Analytically by opening the tube under a titered stannous chloride solution, the excess of oxygen was determined; and by precipitation with barium chloride, the sulphuric acid was ascertained. The ratio obtained was $SO_3 : O :: 10 : 1$; the theoretical ratio $S_2O_7=(SO_3)_2 : O$ or 160 : 16. The same result was reached by simple weighing, the persulphuric oxide formed having a weight of 104 and the constituent sulphurous oxide and oxygen a weight of 94·1+10=104·1, proving it to consist only of sulphur and oxygen. Persulphuric oxide thus prepared may be preserved several days near 0°, but at the end of a fortnight it begins to decompose. Its aqueous solution rapidly loses oxygen, and its solution in concentrated sulphuric acid also loses oxygen but more slowly. It fumes in the air. Its solution in sulphuric acid evolves oxygen rapidly in presence of platinum sponge. Treated with sulphurous acid, hyposulphuric acid is formed. Barium hydrate gives barium persulphate, which is soluble in water.—*C. R.*, lxxxvi, 20, January, 1878. G. F. B.

3. *On the Formation of Ammonium Nitrite in Combustion.*—Zöller and Grete have reëxamined the assertion of Schönbein that ammonium nitrite is formed by combustion, deeming the statements of Carius and Berthelot that ready formed ozone will not oxidize nitrogen irrelevant, since the position of Schönbein is that the nitrite and the ozone are simultaneous products. They found that water which had been produced by the combustion of hydrogen in atmospheric air gave a distinct reaction both with Nessler's test and with potassium iodide, starch and a few drops of acid. Numerous experiments with pure hydrogen—a large gasometer full being burned every day for two months—and pure air, with pure hydrogen and common air, and with unpurified hydrogen, and gave always water containing nitrite. Two to three hundred cubic centimeters of this water distilled from pure baryta, gave a distillate in which platinic chloride produced a precipitate. —*Ber. Berl. Chem. Ges.*, x, 2144, Jan., 1878. G. F. B.

4. *On Fluoranthrene, a hydrocarbon from Coal tar.*—In examining for phenanthrene, a solid hydrocarbon of high fusing point, obtained from coal tar, Fittig and Gebhard found that none of this hydrocarbon was present, but that the mass consisted of pyrene and a new hydrocarbon analogous to it. The two were separated by means of their picrates. The new hydrocarbon is more soluble in alcohol than pyrene, crystallizes in large brilliant plates, which fuse at 109°, and has the composition $C_{15}H_{10}$. The picrate crystallizes from alcohol in long reddish-yellow needles fusing at 182°–183°. On oxidation, it yields a mixture of a quinone and an acid, the latter monobasic, with the formula $C_{14}H_8O_2$. Heated with zinc dust, it gives fluorene, and with lime, diphenyleneketone.

Hence the acid is diphenyleneketonecarbonic acid, with the formula $C_6H_4 \diagdown CO$ / $C_6H_3 \diagdown COOH$, and the hydrocarbon has the constitutional formula $C_6H_4—CH—CH$ / $C_6H_3—\cdot—CH$. The authors give the hydrocarbon, therefore, the name fluoranthrene, calling attention to the fact that Schmitz has proved the identity of Barbier's fluorene with diphenylenemethane. In a postscript, Fittig says that he has just seen the paper of Goldschmiedt upon idryl, and has no doubt that the hydrocarbon $C_{15}H_{10}$ there described is identical with his fluoranthrene.—*Ber. Berl. Chem. Ges.*, x, 2141, Jan., 1878. G. F. B.

5. *Simple synthesis of Formic acid.*—Merz and Tibirca have studied the reaction of carbonous oxide upon caustic alkali, observed first by Berthelot, and find that when a current of this gas is passed over soda-lime at a temperature of about 200° to 250°, the formation of formic acid is rapid. Under favorable conditions an extremely rapid current of gas is completely absorbed, forming an instructive class experiment. This synthesis is important in view of the fact that the hydrogen solidified by Pictet was prepared by fusing together sodium hydrate and formate $NaOH+H.COONa=CO<^{ONa}_{ONa}+H_2$. The fusion took place in the generator.—*Ber. Berl. Chem. Ges.*, x, 2117, Jan., 1878. G. F. B.

6. *Constitution of Nicotine.*—Laiblin has taken another step toward unraveling the constitution of the alkaloids by successfully oxidizing nicotine by means of potassium permanganate, obtaining thereby an acid which he calls nicotinic acid, in colorless needles, fusing at 225°–227° and yielding on analysis the formula $C_6H_5NO_2$, identical with the acid obtained by Weidel by oxidizing with nitric acid. On distillation with lime, the salts of this acid yield pyridine. Hence the acid is pyridine-carbonic acid $C_5H_4N—\cdot—COOH$.—*Ber. Berl. Chem. Ges.*, x, 2136, Jan., 1878. G. F. B.

7. *Chemical Philosophy.*—As recent discussions in the French Academy, together with the paper of Marignac, translated in the last number of this Journal, and the correspondence between Berthelot and Marignac which appears in the present number, may tend to unsettle opinions in regard to the fundamental principles of modern chemistry, it seems important to rehearse in few words certain considerations, which do not appear to have been duly weighed in these discussions. The modern theory of chemistry assumes the recognition of molecules as definite magnitudes, and the law of Avogadro as an accepted principle. It does not attempt to justify these assumptions by chemical evidence, which is confessedly inadequate, but it accepts the principles as definite results of molecular mechanics. This department of physics involves the mechanical theory of heat and the kinetic theory of gases, which have been developed by such masters as Rankine, Clausius, Joule

and Maxwell, and it is not too much to say that these theories—essentially one and the same—rest to-day on as firm a foundation as the undulatory theory of light, and the molecule is a vastly more reasonable conception than that indefinitely attenuated yet indefinitely elastic medium we call the ether. Of the kinetic theory of gases the principle of Avogadro is a necessary consequence and must be true unless this whole theory is a delusion. It is not however at present important to consider whether the absolute truth of the theory will probably be hereafter vindicated or the reverse. For the moment the only question is whether the kinetic theory of gases is a suitable basis for the system of modern chemistry, and we are surprised that any scholar, who appreciates the present position of the theory should not acquiesce in the decision in which the great body of working chemists—certainly of those outside of France—agree. The two great uses of systems, or theories, in science are to direct investigation and to facilitate the acquisition of knowledge. Of the value of the molecular theory in chemistry the history of investigation during the last ten years is sufficient evidence for the first point, and for the second point the paper of Marignac above referred to furnishes abundant indications of the confusion, which would necessarily attend any attempt at the present day to base a system of chemical notation solely on the early crude conceptions of combining proportions or chemical equivalents. As we have written in another place, "Our modern science is a philosophical system based on ideas distinctly stated and consistently developed. . . . . Of course we are far from believing that the ideas now prevailing are necessarily true, and it is perhaps to be expected that our modern school will share the same fate as that which preceded it, but we do believe that the coming system, whatever it may be, will be based on equally clear conceptions, and that in attempting to clarify our ideas and realize our conceptions, we are following the right path and making the only satisfactory progress."

As is well known, although the kinetic theory of gases defines molecular magnitudes, it establishes only their *approximate* relations and even these in but comparatively few cases. It thus gives however the first terms of proportions, whose remaining terms can be supplied by purely chemical evidence, chiefly that obtained by the study of chemical reactions and by quantitative chemical analysis. For a limited number of substances the relative molecular weights as deduced by chemical methods can be brought into comparison with those directly given by the molecular theory, and the agreement is all that could be expected. Indeed, considering the uncertainties which both the physical and the chemical methods involve, and our great want of knowledge of the changes of molecular structure, which evidently very constantly attend the volatilization of a solid substance, it is surprising that the discrepancies are not more numerous than those which have as yet appeared; and to abandon our clear conceptions and hitherto safe guides on account of a few anomalies,

which remain to be explained, would be as foolish as for a navigator to throw away his chart and compass because he could not fully account for all the deviations of the magnetic needle.

In a recent paper, of which a notice will be found in the previous number of this Journal, M. Berthelot, after referring to the well known principle that the tendency of a chemical system is toward that composition which corresponds to the maximum evolution of heat, writes—"the secret of the decomposition of baric perhydrate is not to be found in any symbolic considerations drawn from a figurative arrangement of atoms, but is explained by very simple and very obvious principles, resulting from the regular action of molecular mechanics." The tone here taken in regard to the study of structural chemistry, which was also heard in the debate in the French Academy, greatly surprises us. Certainly these eminent French chemists must know that in Germany the study of chemistry has been during the last ten years almost solely directed by theories of molecular structure, and that during this period the German chemists, thus guided, have done more to advance their science than all the other nations of the world combined. Moreover, if there are such things as molecules, and if the present science of "*molecular mechanics*" is a legitimate branch of knowledge, then the so-called "Law of Avogadro" is an established principle, and then also molecular structure is a necessary inference at least in the only sense in which it is held by the great masters of modern chemical philosophy.

J. P. C., JR.

8. *The Laws governing the Formation of Ethers.*—N. MENSCHUTKIN recently presented to the Russian Chemical Society a paper of great general interest, as it is one of the first fruits of the comparative method of research which has already yielded such brilliant results in the hands of Bogusky. His work relates to the formation of the acetic ethers from sixteen different alcohols, and is in part a revision of that of Berthelot and Pean de St. Gilles, who were the first to enter this field, but whose researches, as the science was not ready for them, have lain neglected since 1863. The position of the hydroxyl group, he finds, has a marked effect on the etherification, the rate during the first hour being from twenty to thirty per cent faster, during the following hours somewhat slower for primary alcohols (characterized by CH_2OH) than for secondary (characterized by CHOH), while the limit, that is, the whole amount of ether which it is possible to form from secondary alcohols, varying from 61·5 to 66·65 per cent of the theory, is lower than that of the primary, 69·61 to 82·24 per cent; differences of molecular weight within an homologous series do not affect the initial rate, which is the same for all its members with a few easily explained exceptions, but do influence the limit, which increases about two per cent in the primary series, about 1·1 per cent in the secondary with each increase of CH_2; the initial rate and limits of unsaturated alcohols are lower than those of the saturated, the rates differing by about ten per cent, and—the

most curious fact observed in this research—benzyl and cinnamyl alcohols have the same initial rate as allyl alcohol in spite of the wide differences between the aromatic compounds and those of the fat series, which leave little in common to these substances except their alcoholic nature: it follows from these facts that new alcohols can easily be referred to their proper class by determining their initial rate of etherification. Menschutkin's somewhat lengthy discussion of the relative rate, i. e., the rate referred to the limit seems rather unnecessary, as it is of course a mere function of the limit, but it is not worth while to dwell on the single blemish in a paper so full of interesting observations, many of which cannot even be mentioned in this notice from want of space, and of valuable suggestions for future research, notably those in regard to the bearing of isomerism on the rate and limit, which it is to be hoped he will follow out thoroughly, as soon as he has finished the tertiary alcohols (containing COH) with which he is now at work.—*Ber. Deutsch. Chem. Ges.*, x, 1728, 1898. C. L. J.

9. *Solid Hydrogen, Density of Liquid Oxygen.*—With the apparatus described in the last number of this Journal, M. PICTET has succeeded in both liquefying and solidifying hydrogen, and has obtained a product which is described as having a metallic appearance, and justifying the opinion long since expressed by Dumas, that hydrogen is a *gaseous metal.* M. Pictet prepared the hydrogen by heating potassic formiate with potassic hydrate in the "shell" of his apparatus (page 139). The pressure increased gradually and in a little more than half an hour it reached 650 atmospheres. At this point the pressure remained stationary for some seconds, when "the stop-cock was opened and a jet of a steel-blue color escaped with a hissing noise." "This jet suddenly became intermittent, and the spectators observed a hail of solid corpuscles projected with violence on the ground, where they produced a crackling noise."

M. Pictet also announces the result of a rough estimate of the density of liquid oxygen based on many factors, but *chiefly* on the dimensions of his apparatus, and "the variation of pressure corresponding to the condensation of oxygen in the tube immersed in the carbonic acid." This variation corresponded to 74·26 atmospheres, and from this he deduces that the tube whose volume was 46·25 cubic centimeters held 45·467 grams of the liquid gas. Assuming that the tube was full, the density of liquid oxygen must be nearly the same as that of water, and if, as is probable, the tube was not quite filled, the agreement is still closer. This density is about one-half of that of sulphur, and the result is regarded by Pictet as a most complete experimental verification of a prediction of Dumas based on the inference that the atomic volumes of oxygen and sulphur must be equal when in the same state of aggregation. As the atomic weight of oxygen is one-half of that of sulphur, Dumas had concluded that the densities of the two substances would bear to each other the same ratio.

In Nature for Jan. 31 may be seen a large wood-cut represen-

tation of the apparatus used by M. Caillelet in his experiments described in our last number. J. P. C., JR.

10. *The effect of Light and Heat upon the electrical resistance of Selenium.*—W. SIEMENS has undertaken a long research upon the photo-electric properties of selenium and concludes that its sensitiveness to light is a peculiar property of this metal, and finds that it is not a general property of metals as has been claimed by Bornstein. Siemens points out the use of selenium as a photometer for lights of all colors. The photometers now in use do not admit of the comparison of colored lights with much precision. When we pronounce that two lights of different colors are equal, our decision is a subjective one. The selenium photometer, however, gives unmistakable indications for the strength of lights of all colors. Unfortunately there is no means at present of graduating and obtaining the values of the scale divisions of such a photometer. Siemens has endeavored to obtain an empirical scale, but without much success. He hopes, however, that other observers with better means may be more successful.—*Ann. der Physik und Chemie*, No. 12, 1877, p. 521. J. T.

11. *On the Influence of Light upon the electrical resistance of Metals;* by G. HAUSEMANN.—The author has submitted the results of Bornstein upon the above subject to proof. He used a very sensitive galvanometer and a thermo-electric element of iron and copper from which, by keeping the ends at a constant difference of temperature, he was enabled to obtain a constant electromotive force. When the metals were intercalated in the circuit and their resistance measured, no difference was observed which could be attributed to the effect of light. Another method depending upon damped vibrations was used with the same result, and Hausemann therefore concludes that Bornstein is in error in attributing the effects he obtained to light. In another paper he discusses the results obtained by Bornstein in photo-electricity and fails to confirm the results of that investigator.—*Ann. der Physik und Chemie*, No. 12, 1877, p. 521. J. T.

12. *On Electromagnetic and Calometric absolute measurements.*—Prof. H. T. WEBER in a paper not yet concluded, discusses the various results, obtained by different observers for the value of Siemens' resistance unit, and concludes from three independent sets of measurement that the divergent results obtained by MM. Wilh. Weber, F. Kohlrausch and L. Lorenz, are affected with errors of observation. Prof. H. T. Weber finds for the value of Siemens' resistance unit the expression

$$1 \text{ S. M. U.} = 0{\cdot}9550 \times 10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right)$$

Weber concludes his first paper as follows:—

"(1.) The fundamental laws hitherto recognized of induced currents of variable intensity represent with great precision the real facts. The opinion of M. Lorenz, that the great difference between the results found by M.M. Weber, F. Kohlrausch and the physicists of the British Resistance Committee was the conse-

quence of our imperfect knowledge of the laws of such induced currents, finds no confirmation at all in the above experiments.

(2.) Absolute measurements of resistance can with the means of galvanic observations nowadays at our disposal, be carried out with such exactness and certainty as can only be attained in few departments of physics. The notion widely accepted among physicists, that absolute measurements of resistance belong to those physical measurements which are capable of giving only roughly approximate values, and require peculiarly equipped localities for carrying them out—an opinion to which W. Siemens, among others, has given expression in the sentence 'we may certainly pronounce positively that even the most practised physicists, supplied with the most perfect instruments and localities, will not be able to make absolute resistance measurements that would not differ by some percentage'—is refuted by the above results of experiment. According to my experience, these measurements can be effected with tolerable accuracy with very scant means and in moderately equipped localities."—*Phil. Mag.*, Jan., 1878, p. 30. J. T.

13. *Notes from the Chemical Laboratory of the Johns Hopkins University*, Nos. 4–8. 22 pp. 8vo. Baltimore, December, 1877.—No. 4, on the oxidation of Bromethyl-toluene and of similar substitution-products, by H. N. Morse and I. Remsen; No. 5, on the oxidation of xylenesulphonic acids, by M. W. Iles and I. Remsen; No. 6, on acetylamidophenols, by H. N. Morse; No. 7, a correction, by I. Remsen; No. 8, a Lecture-experiment, id.

II. Geology and Mineralogy.

1. *Annual Report of the Wisconsin Geological Survey for the year* 1877; by T. C. Chamberlain, Chief Geologist.—Besides valuable notes on the local geology of portions of the State, this Report contains brief descriptions of new Silurian fossils of Wisconsin. The Potsdam sandstone afforded the species *Bellerophon antiquatus*, and new species of each of the following genera of Trilobites: *Conocephalites*, *Crepicephalus*, *Ptychaspis*, *Agraulos*, *Arionellus* and *Elliptocephalus*; and the Lower magnesian limestone trilobites, of the genera *Dicellocephalus* (near *Bathyurus* of Billings) and *Illænurus*, beside *Euomphalus*, and species of *Scævogyra*, *Metoptoma* and *Leptæna*. A number of new species also are described from the Trenton, Hudson River and Niagara groups. The Report closes with a biographical notice of Moses Strong, who had been one of the assistant geologists of the Survey.

2. *Geological Survey of New Jersey.* Annual Report of George H. Cook, State Geologist, for the year 1877.—The final report on the clay district of Middlesex County, states that the yield of clay in some parts is 10,000 tons an acre, and single acres have yielded 40,000 tons; that in a single year 265,000 tons of fire clay and 20,000 of stone-ware clay have been dug; and there is no reason why this amount should not be quadrupled.

Professor J. C. Smock gives a valuable account of the drift of New Jersey, with the courses of the glacial striæ. The direction is *southwest* on the western slope of the Blue Mountain in Sussex and Warren Counties, at the zinc vein of Mine Hill in Franklin, on the west slope of Palisade Mountain at Bergen Hill, on First and Second Mountains west of Orange, and some other points; it is *southeast* on the top of Palisade Mountain at various points from Alpine to Newark and mostly between S. 15° E., to S. 25° E.; and nearly east-and-west (S. 60° E. to S. 80° E.) on the western slope of Blue Mountain along Greenville turnpike, N. Y., at High Point, Blue Mountain, on eastern slope of Blue Mountain southwest of Branchville, and on First and Second Mountain west of Paterson. The southern edge of the drift in New Jersey is stated to pass along the line of Short Hills extending from Perth Amboy, on the north side of the Raritan, to the First Mountain. Between First and Second Mountains it fills the valley for less than half a mile south of the Morris and Essex railroad. It may thence be traced to Long Hill, Morristown, Dover and on to the Delaware below Belvidere. "The whole line of this moraine is remarkably plain and well defined."

3. *Bulletin of the United States Geological and Geographical Survey;* F. V. Hayden, Geologist in charge. Vol. iv, No. 1.—This number of the Bulletin contains papers by G. B. Sennett on the Ornithology of the Rio Grande; by E. D. Cope on Cretaceous and Tertiary fishes, and on Professor Owen's remarks on the Pythonomorpha; by V. T. Chambers on new Tineina, etc., from Texas; by A. R. Grote on Noctuidæ, chiefly from California; by J. S. Kingsley on N. American species of Alpheus; by Dr. C. E. McChesney on the Mammals of Fort Sisseton, Dakota; by R. Ridgeway of the American Herodiones; by S. H. Scudder on the Butterflies of Southern Utah and Arizona; by Drs. Coues and Yarrow on the Herpetology of Dakotah and Montana; by Dr. E. Coues on the Consolidation of the hoofs in the Virginian deer, and on a breed of solid-hoofed pigs apparently established in Texas.

In the case of the deer the consolidation is confined to the horny substance of the true hoof.

With regard to the pigs of Texas, the *animal is completely solidungulate*, and the anchylosis of the terminal phalanges of the toes has produced a single broad phalanx in the axis of the limb. Above this, however, the other two phalanges, medial and proximal, of each of the two principal digits, remain perfectly distinct. The hoof is perfectly solid and on its sole there is a broad angular elevation of horny substance which is curiously like the frog of the horse's hoof. The breed is so firmly established that no tendency to revert to the original and normal form is observable. Mr. G. W. Marnock, of Helotes, Bexar Co., Texas, who sent specimens and facts to Dr. Coues, states that in the cross of a solid-hoofed boar with a sow of the ordinary type, a majority of the litter have the peculiarity of the male parent.

Dr. Coues states that there is here a change from an artiodactyle (or even-toed) mammal to a perissodactyle (or odd-toed). This is true if we look only at the literal meaning of the word perissodactyle. But in actual structure, the animal is no less artiodactyle than before; for the two toes are still present as much as before, although coalesced at extremity; and the *fourth*, the *outer* of the two, *equals the third*—which is the grand characteristic of artiodactyles and determines that even stroke of the foot to which is due the possibility of the coalescence of the two metacarpal or metatarsal bones into a "cannon" bone. J. D. D.

4. *Report on the Geographical and Geological Survey of the Rocky Mountain Region;* by Major J. W. Powell, in charge of the survey. 20 pp., 8vo.—This report is a brief and very general statement of the work done by Major Powell's expedition in 1876. The region investigated lies mainly within the Territory of Utah. The work of the primary triangulations was under Professor A. H. Thompson and the topographic in two parties, under Mr. W. H. Graves and Mr. J. R. Henshawe. Geological investigations were carried forward by Major Powell, Mr. J. K. Gilbert and Captain C. E. Dutton. The results also include researches in the departments of Botany and Ethnography, besides the taking of photographic views, and an investigation of the climate of Utah with reference to the rise and fall of Great Salt Lake.

5. *On the variations of the Decorticated Leaf-scars of certain Sigillariæ; and on the variations of the Leaf-scars of Lepidodendron aculeatum* Sternberg; by Herman L. Fairchild.—These two papers from the Annals of the N. Y. Academy of Science, are illustrated by several excellent plates, showing the variety of forms which occur in the markings of one and the same species.

6. *Elements of Geology;* a text-book for Colleges and the general reader; by Joseph LeConte, author of "Religion and Science," and Professor of Geology and Natural History in the University of California. 588 pp. 8vo, with numerous wood cuts. —Professor LeConte is well known to the readers of this Journal through his papers on binocular vision, the glaciers of the Sierras, the lava floods of the Cascade Mountains and other parts of the Pacific slope, a theory of the origin of the earth's features, and on a theory with regard to periods of catastrophe being "critical periods" in the development of species, besides some other topics. His work on Geology opens with the Dynamical department of the science, taking up first the subjects of atmospheric and aqueous agencies and continuing with those of volcanoes and earthquakes, and gives with ability and clearness the facts and principles. Part II, on Structural geology, treats of the structure and positions of stratified and igneous rocks, the arrangement and condition of fossils, the classification or chronological order of strata, mineral veins, and the structure, origin and erosion of mountains. Part III is occupied with Historical geology. The illustrations are numerous and well selected.

7. *Contributions to the Fossil Flora of the Western Territories.* Part II. *The Tertiary Flora;* by LEO LESQUEREUX. 1877. pp. 366, and 65 lithographic plates, 4to.—This noble and important volume, one of the latest and ripest fruits of Dr. Hayden's Geological and Geographical Survey, is just issued, and at this moment we can only announce its publication. A critical account of it may in due time be given, either from the paleontological or botanical point of view, or from both.

Also the paper on *Land Plants* recently discovered *in the Silurian rocks of the United States*, published in the Proceedings of the American Philosophical Society, of October last (with a plate) deserves particular mention. The discovery of some of these remains was recorded in this Journal, for January, 1874; but new materials and new studies have confirmed Mr. Lesquereux in his opinion of their character. Meanwhile Count Saporta has reported the finding of a branch of a Fern in the Silurian slates of Angers, France, announcing at the same time the prior American discovery. A. G.

8. *The rocks of the "Chloritic formation" on the Western border of the New Haven region;* by GEORGE W. HAWES.—In a contribution from the Sheffield Laboratory* on the above named subject, the writer gave analyses of several specimens of rock from the so-called "Chloritic formation," and pointed out the resemblance in composition between these rocks and basic igneous rocks. By the aid of the microscope the rocks were inferred to be pyroxenic. On a further examination with better facilities, I have found that the mineral, supposed to be pyroxene, is hornblende, and that the rocks belong therefore to the diorite group. These rocks present many very interesting microscopic peculiarities, among which are some that distinguish them very well from the eruptive diorites, and I refer those who are interested in the subject to the forthcoming volume of the New Hampshire geological survey, in which analogous rocks are discussed and illustrated, and in which a revision of my previous study† upon these rocks will be found.

9. *Elevated Quaternary beds of Grinnell Land and North Greenland.*—The December number of the Annals and Magazine of Natural History contains an article on these evidences of former Arctic subsidence and elevation, by H. W. FEILDEN, naturalist to the late Arctic Expedition. The author speaks of his communication as a supplement to a paper on the same subject by Dr. Gwyn Jeffreys, in the September number of the same Journal; and he alludes also to a memoir by Mr. Henry H. Howorth, published in the Journal of the Royal Geographical Society for 1873, in which the subject of the elevation of the land in the Arctic Regions is "treated in a very able and comprehensive manner." He states that his observations sustain the opinion of Mr. Howorth, that "the land which surrounds the North Pole is undergoing a general movement of upheaval; or, to be perfectly correct,

* This Journal, Feb., 1876. † Ibid., Aug., 1876.

we find on it, in all directions, evidences that there has been a movement of upheaval since there was any subsidence." In Grinnell Land and North Greenland shell beds in some localities rest on "Miocene strata, and extend to an elevation of not less than a thousand feet above their level," proving, as he states, that since the Miocene there has been "a subsidence of over a thousand feet, and a subsequent upheaval to a similar altitude." The beds overlie also the other rocks of the country. The shells are largely the same that occur in the Champlain deposits of the St. Lawrence River and the Labrador and Maine coasts: such as, *Saxicava rugosa*, *Mya truncata*, *Cardium Islandicum*, *Tellina calcaria*, *Astate borealis*, *Pecten Grœnlandicus* and others. The paper particularly describes the several localities, and is followed by a note by Mr. Jeffreys, giving a list of the fossils observed and their localities.

10. *A Seal from the Leda Clay (Champlain beds) of the Ottawa valley.*—Dr. Dawson describes this seal as the common Greenland species, *Phoca Grœnlandica*. It occurs in the clays of Green's creek, which have "afforded beautiful specimens of the Capelin and other fishes, and also of marine shells of northern and cold-water types."—*Canadian Naturalist.*

11. *Notes on the Mineralogy and Petrography of Boston and vicinity;* by M. Edwards Wadsworth.—The rocks examined by Mr. Wadsworth are all from the vicinity of Boston. They are proved to belong to the doleritic type, although they have in the past been classed by different authors under various names as "diorytes," "syenites," etc. The microscopical examination has shown that the normal and essential constituents of the rocks are augite, feldspar, and magnetite; they contain also apatite, olivine, pyrite, hornblende, and perhaps biotite. The rocks from most of the localities are marked by a greater or less degree of alteration, in some the change being slight, while in others the decomposition-product "viridite" forms with magnetite the mass of the rock. The rocks which have previously been called "diorite" and "trap" are considered to be identical and of the same age, while the "greenstones" which show more alteration are regarded as older. The localities for the different rocks examined, are stated, and the microscopical characters given with care and minuteness.

Mr. Wadsworth gives in the beginning of his paper a list, covering six pages, of the published articles upon subjects connected with the mineralogy and geology of Eastern Massachusetts.—*Proc. Boston Nat. Hist. Soc.*, xix, 217, May, 1877.

12. *Analysis of Samarskite from Mitchell Co., North Carolina;* by Prof. C. F. Rammelsberg.—The North Carolina samarskite has already been analyzed by Miss E. H. Swallow, Prof. O. D. Allen and Dr. J. Lawrence Smith (see this Journal, III, xiii, 362, and xiv, 130). Prof. Rammelsberg, who has contributed so much to our knowledge of this group of minerals, has also published an analysis of the same mineral. It is as follows:—

Cb_2O_5	Ta_2O_5	SnO_2	UO_2	$Fe_2O_3(Mn_2O_3)$	Ce_2O_3*	Y_2O_3	ErO	SiO_2
41·07	14·36	0·16	10·90	14·61	2·37	6·10	10·80	0·56

* With a little Di. =100·93

The formula deduced from the above is $8R_2Nb_6O_{21}+R_2U_5O_{21}$ where $R=Y_2$, Fe_2, $Ce_2(Er_2)$, each double atom having an equivalence of six (Y=92, Ce=138). The American samarskite differs from the Uralian mineral in the high percentage of tantalic acid, and of the element erbium.—*Ann. Phys. u. Chem.*, II, ii, 663.

III. Botany and Zoology.

1. *Supplementary Note to the Review of Darwin's "Forms of Flowers."* (In January No, pp. 67–71.)—A contributor to the Bulletin of the Torrey Botanical Club, having advanced the idea that the blossoms of *Gentiana Andrewsii* were cleistogamous, because generally seen with corolla closed, we mentioned: 1, that the corolla opened in bright sunshine for a short time, also that humble bees (as others had also recorded) bodily entered even the closed flowers, and would therefore cross-fertilize them: 2, that there was a neat adaptation for ulterior self-fertilization; the pollen long remaining fresh on the ring of extrorse anthers, in such position that when the stigmas of the flower tardily matured, diverged, and became revolute, a part of the stigmatic surface commonly came into contact with the abundant pollen; but this only some time after exposure to the chance of a pollenized entering bee. In the Torrey Bulletin for December last, (vi, 189), Mr. Meehan follows this up with some observations and with other statements which, on account mainly of the singular deductions, may call for a brief remark. He states that *Gentiana Andrewsii* in his neighborhood behaves differently, and that the flowers "do not last a long while." Between this and "a rather long while," the discrepancy is not very explicit, and it is more than done away with by the statement following, that "the ovarium, however, continues to grow, and soon pushes itself through the mouth of the corolla, exposing the stigmatic surfaces which remain in a receptive condition for some time after exposure." This is equivalent to saying that anthesis lasts for a week or two, which is certainly a long while, and doubtless too long. For we are confident that when the ovary, or rather the maturing capsule, is thus exserted out of the mouth of the fading corolla, the stigma no longer "remains in a receptive condition." If it has not been cross-fertilized before this, its day is long passed.

Then follows this: "The only difficulty with me is, that I do not see where the pollen to cross-fertilize is to come from. Mr. Darwin teaches that pollen from the same plant, or from plants growing under similar conditions, is practically no cross-fertilization." This is equivalent to saying that there is no "practical" (meaning useful) cross-fertilization if the plants grow near enough for a bee to fly from the one to the other; which is making what "Mr. Darwin teaches" extinguish cross-fertilization effectually!

Then, "But with me, bees or other insects do not go into one flower on one plant, and then away to another many yards away, then returning, and again going back, continuously going and coming, as a zealous cross-fertilizer, so beautifully arranged by nature, should do, . . . whatever they may do elsewhere." Certainly only the bees in the writer's bonnet behave in this way, or were ever thought to do so.

The article continues thus: "However, it is well to recognize the fact, that plants, and no doubt insects, behave differently in different places. For instance, Mr. Darwin, from English experiments, utterly denies that *Linum perenne* can fertilize itself with its own pollen. He says we may as well "sprinkle over it so much inorganic dust. But a single plant which I brought with me from Colorado, in 1873, bears fruit freely in my garden every year. It shows that how a plant may behave in one place, is no rule as to how it will elsewhere." This extremely remarkable induction of a general rule,—that plants and insects cannot be depended upon for behavior,—is inferred from two instances, one of which has been sufficiently examined; and now a few words may dispose of the other. Mr. Meehan must have noticed (in Forms of Flowers, p. 92) that Darwin's result has been completely confirmed by Hildebrand; and he might have read, on p. 100, the statement, taken from Alefeld, that no American species is heterostyled; and on p. 100, that the Colorado plant, *Linum Lewisii*, of Pursh, the American representative of *L. perenne*, is suspected to be a distinct species, of a sort fully capable of self-fertilizing. This is what Mr. Meehan's observation goes to prove; and so, instead of showing that the behavior of species cannot be relied on, he has unwittingly brought evidence of the correctness of Mr. Darwin's surmise. We looked upon Mr. Meehan's little article as a piece of pleasantry, and should not have referred to it if it had not been noticed abroad as something serious. A. G.

2. *Historia Filicum; an Exposition of the Nature, Number and Organography of Ferns, &c.* By JOHN SMITH, A.L.S., Ex-Curator of the Royal Gardens, Kew, etc. London: Macmillan & Co., 1875; re-issued 1877. 12mo, pp. 429. And with 30 lithographic plates.—The title page proceeds to state, that this volume contains a review of the principles upon which genera are founded, and the systems of classification of the principal authors; with a new general arrangement; characters of the genera; remarks on their relationship to one another, their species, reference to authors, geographical distribution, &c. The plates, drawn on stone by Fitch, illustrate the tribes and leading genera. Mr. Smith is, perhaps, the oldest living pteridologist, and while he had his eye-sight was one of the best. No one else was so intimately and extensively acquainted with Ferns in a living state. In him unusual practical knowledge was combined with no mean talent for systematic arrangement. He was, next to Presl, the first to use the characters of venation, which Brown had cautiously suggested for the definition of genera, and he may be said

to use them with better judgment than Presl, upon an ampler store of materials. His papers, published in 1841–1843, were thought very valuable; and he has since endeavored to make his arrangement more natural. Having nearly a thousand species of Ferns growing under his care, he was led to study their mode of growth, especially of the caudex, and the disposition of the fronds upon it. He finds three distinct modes or types, which we cannot in this brief notice explain, upon which he founds main divisions in classification. One of them is characteristic of an already well-marked primary group, the *Marattiacæ;* the other two designated as *Eremobrya* and *Desmobrya*, rather neatly, but not so absolutely, divide the typical true Ferns into two great divisions. Succeeding systematists may not assign to these characters so high a place, but they cannot overlook them.

The present handy volume does not take the place of a *Species Filicum*. It merely gives the names of the leading species, "the chief aim of the work being the definition of genera, and their classification, founded on the different modes of growth, venation, and fructification." It has good indexes, a list of Fern authors, the etymology of generic names, and is altogether a useful book both to botanists and amateurs. To those who do not possess a considerable botanical library, it would appear to be indispensable. A. G.

3. *Ferns of North America.* By Prof. DANIEL C. EATON, Part 2.—This prompt continuation argues well for the enterprise, and plates and letter-press are every way commendable. The latter extends to p. 43. The three plates illustrate, *Asplenium ebeneum*, the ambiguous *A. ebenoides*, *Botrychium Lunaria*, *lanceolatum*, and *boreale*, *Cheilanthes lanuginosa* and *C. California*, the letter-press of the latter not included. Referring to the remark at the close of the account of *Asplenium ebeneum*, we suggest that the best of all reasons for adhering to this specific name, is that it is the earliest given for this *Asplenium* as such. A. G.

4. *List of Fungi found in the vicinity of Boston*, part 2, and *Remarks, &c.* From *Bulletin of the Bussey Institution*, vol. ii, pp. 224–252.—The principal bulk and chief interest of this article will be found in what are called the "Remarks," which are critical studies of development, synonymy, &c., of various interesting minute Fungi, beginning with the *Chytridineæ*, unicellular parasites, inhabiting the cells of both aquatic and land plants. By a study of their development, Prof. Farlow shows that a common parasite on *Amphicarpæa*, which has passed for an *Uredo*, and figured under various names, is a *Synchitrium*, a chytridineous genus, and seemingly not distinct from *S. fulgens*, found on *Œnothera biennis*. The only new species described is of this genus; but, what is better for science, a goodly number of species are reduced and properly referred; for instance, *Exobasidium Vaccinii* is made to include *E. Azaleæ* and *E. Andromedæ* of Peck, and *E. discoideum* of Ellis. The synonymy of various species of *Uromyces* is given in detail, and various newly proposed species are referred to Schweinitzian originals. A. G.

5. *Journal of the Linnean Society.*—The last two numbers, containing together almost 200 pages, are filled with the *Spicilegium Floræ Maroccanæ*, by John Ball, based on the collections made in Marocco by Sir Joseph Hooker and himself; a critical study, illustrated by plates of some new or rare species. Thus far it reaches the beginning of the *Umbelliferæ*. A. G.

6. *Guide du Botaniste in Belgique* (*Plantes vivantes et fossiles*), par FRANCOIS CRÉPIN. Bruxelles and Paris, 1878. pp. 495. 18mo.—A handbook for the tyro in Botany, giving him just the information he most needs, and which, without such help at the outset, he will be a long while in obtaining;—details for herborization; preparation of specimens; formation of herbarium; arrangement for exchanges; nature and characteristics of different sorts of botanical books, and how to use them; how to write them, moreover; and a useful section on the correction of proofs; the botanist's library and what it must needs contain; selection of some of the principal floras and botanical periodicals of the day; a choice of European *Exsiccatæ;* sketch of the principles and methods of vegetable paleontology, with detailed instructions for collecting and managing specimens; systematic lists of the more important works on fossil plants, etc. This all belongs to the first part of the volume, and is general. The second part gives the history of Botany in Belgium, beginning with Jean de Saint-Arnaud of Tournai (born in the year 1200), and coming down to the present day; an account of botanical instruction in Belgium; of the Botanic Gardens, Scientific Societies and Libraries, Herbaria, etc.; then the geographical botany of the country; a catalogue of its fossil flora; full instructions for herborizations in different parts of Belgium, with enumeration of the choice plants of each district; the same for the fossil flora; and, finally, a catalogue of all the works of Belgian botanists, or those who have sojourned in Belgium long enough to be so accounted, from Anselme de Boodt, 1640, and Clusius, downward. No other country has such a hand-book. A. G.

7. *A curious adaptation to insect-fertilization in Trichostema* is described in a letter from Mr. I. J. ISAMAN, of Bangor, California, in a letter to Prof. Bessey of Iowa Agricultural College. The species is *T. lanceolatum*, a very common California annual. Mr. Isaman sends drawings of the two states of the blossom to illustrate his account of the process, which he describes as follows:

"The tube of the corolla is bent upon itself when in its normal condition. On inserting a pin or a small splint, the tube is straightened, and the stamens and pistil are thrown forward, and strike very forcibly upon the back of any intruding insect. I have watched bees for hours, gathering honey from these plants, and have been very much amused by the performance."

The sketches well show how the operation may take place. But we must ask our Californian botanists to verify it, and to ascertain the character and cause of the movement or change

of position; also to send us seeds for raising the plant; for this species is not in cultivation. Our two Eastern species should likewise be examined in this regard. A. G.

8. *Botanical Necrology of* 1877.—This contains an unusual number of noted names. The following are the principal.

Mrs. Maria Emma Gray, widow of the late Dr. J. E. Gray, died, at the age of 90, a little before the last year began, viz: Dec. 9, 1876, but her name was not in our last necrology. She was a keen student of *Algæ*, in which order her name is commemorated by the genus *Grayemma*, established by her husband.

Pleasance Smith, widow (since 1828) of Sir James Edward Smith, although not herself a botanist, should have a place of honor in the list. She died Feb. 3, 1877, at the age of 104 years! A short obituary notice appeared in this Journal, April last.

Guiseppe de Notaris, long Professor of Botany at Genoa, translated to Rome in 1872, a cryptogamic botanist of much celebrity, died January 22, 1877, in his 71st year.

Wilhelm Hofmeister, Professor at Tübingen, a most distinguished vegetable anatomist, died, January 12, 1877, in the 53d year of his age. His death was noticed in last year's necrology.

Alexander Braun, Professor of Botany at Berlin University, one of the best botanists of the age, died, March 29, 1877, in his 72d year. An obituary notice was duly given in this Journal, in the June number.

E. Bourgeau, one of the best and most persevering botanical collectors of our day, who crossed the American continent at the north with Palisser, and made his last collection in Mexico during the French occupation, died in France early in the year.

Themistocle Lestibudois, formerly Professor of botany at Lille, where he succeeded his father and grandfather, and who was an author of considerable note in his day, died last summer at the age of 80.

Hugh Algernon Weddell, English in birth, French by adoption, author of the unfinished *Chloris Andina*, and of monographs of the *Urticeæ*, *Podostemaceæ*, and *Cinchona*, a first-class systematic botanist, died in August last, of heart-disease, at the age of 58.

Filippo Parlatore, Professor and Curator of the Museum at Florence, author of a *Flora Italiana*, of a monograph of the species of Cotton, of the *Coniferæ* in DeCandolle's Prodromus, and other works in systematic botany, died at Florence, September 9, at the age of 61.

Prof. John Darby, author of a Botany of the Southern United States, died in August last, at the age of 73. An obituary notice appeared in the December number of this Journal. A. G.

9. I. *Desmidieæ et Œdogonieæ;* by O. Nordstedt and V. Wittrock. II. *Bohusländs Œdogonieer.* III. *Nonnullæ Algæ aquæ dulcis Brasilienses;* by O. Nordstedt.—The three papers just named appeared in the Proceedings of the Academy of Science of Stockholm, and each is accompanied by a plate. The species described in the first named paper were collected by

Nordstedt himself in Italy and the Tyrol. The desmids were determined by Nordstedt and the Œdogoniæ by Wittrock. The species from Brazil, which with two exceptions are desmids, were collected by A. Glaziou and E. Warming. W. G. F.

10. *A new Species of Chimæra from American Waters;* by T. GILL.—One of the most unexpected discoveries recently made in American ichthyology is that of a species of the genus *Chimæra*, of which a specimen has lately been sent to the Smithsonian Institution. It was caught southeast of the La Have bank, in lat. 42° 40′ N., lon. 63° 23′ W., at a depth of 350 fathoms, with a bait of halibut. A close comparison of the specimen with individuals of the European *Chimæra monstrosa* renders it evident that it does not belong to that species, but is an entirely distinct specific form. It may be named *Chimæra plumbea*, and diagnosed as follows:—A *Chimæra* with the snout acutely produced; the ante-orbital flexure of the suborbital line extending little above the level of the inferior margin of the orbit; the dorsals close together; the dorsal spine with its anterior surface rounded; the ventrals triangular and pointed; the pectorals extending to the outer axil of the ventrals; and the color uniformly plumbeous. By these characters the species is readily separable from the *Chimæra monstrosa* and other species of the genus.—*Proc. Phil. Soc.*, Dec. 22, 1877, Washington.

11. *Dr. W. M. Gabb on Dr. Warring's paper on the growth-rings of exogenous plants a proof of alternating seasons*, in the last volume of this Journal, page 394. (From a letter to the Editors, dated Santo Domingo, Dec. 30, 1877.)—Dr. Warring's paper, in the November number of the Journal, raises a question that I, in my ignorance, thought had been settled long ago. I have lived so long in the tropics that I feel almost as if I had a right to speak "as one having authority." I have had abundant means of observation, and am thoroughly convinced that the conclusions arrived at by the Doctor are correct. I can only demur at one: I have never seen an exogen without rings (3).

The mahogany tree grows here at the average rate of one inch diameter per annum. Now the one-half inch radius certainly contains an average of three rings—which cannot in any manner correspond to wet and dry seasons, to variations of temperature, nor yet to the time of the fructification of the plant.

I might go farther: Santo Domingo is eighteen to nineteen degrees north of the Equator. But I have seen the same facts amply proven ten degrees farther south, where there is practically no variation of temperature, where the rainy season lasts twelve months in the year, and where the trees are, to all appearances, equally vigorous at all seasons of the year.

IV. ASTRONOMY.

1. *On the age of the Sun in relation to Evolution;* by JAMES CROLL.—One of the most formidable objections to the theory of evolution is the enormous length of time which it demands. On

this point Professor Hæckel, one of the highest authorities on the subject, in his "History of Creation," has the following:—"Darwin's theory, as well that of Lyell, renders the assumption of immense periods absolutely necessary. . . . If the theory of development be true at all there must certainly have elapsed immense periods, utterly inconceivable to us, during which the gradual historical development of the animal and vegetable proceeded by the slow transformation of species. . . . the periods during which species originated by gradual transmutation, must not be calculated by single centuries, but by hundreds and by millions of centuries. Every process of development is the more intelligible the longer it is assumed to last."

There are few evolutionists, I presume, who will dispute the accuracy of these statements; but the question arises, does physical science permit the assumption of such enormous periods? We shall now consider the way in which Professor Hæckel endeavors to answer this question and to meet the objections urged against the enormous lapse of time assumed for evolution.

"I beg leave to remark," he says, "that we have not a single rational ground for conceiving the time requisite to be limited in any way. . . . It is absolutely impossible to see what can in any way limit us in assuming long periods of time. . . . From a strictly philosophical point of view it makes no difference whether we hypothetically assume for this process ten millions or ten thousand millions of years. . . . In the same way as the distances between the different planetary systems are not calculated by miles but by Sirius-distances, each of which comprises millions of miles, so the organic history of the earth must not be calculated by thousands of years, but by palæontological or geological periods, each of which comprises many thousands of years, and perhaps millions or milliards of thousands of years."

Statements more utterly opposed to the present state of modern science on this subject could hardly well be made. Not only have physicists fixed a limit to the extent of time available to the evolutionist, but they have fixed it within very narrow boundaries.

Every one will admit that the organic history of our globe must have been limited by the age of the sun's heat. The extent of time that the evolutionist is allowed to assume depends, therefore, on the answer to the question, what is the age of the sun's heat? And this again depends on the ulterior question, from what source has he derived his energy? The sun is losing heat at the enormous rate of 7,000 horse-power on every square foot of surface. And were it composed of coal its combustion would not maintain the present rate of radiation for 5,000 years. Combustion, therefore, cannot be the origin of the heat.

Gravitation is now almost universally appealed to as the only conceivable source from which the sun could have obtained its energy. The contraction theory advocated by Helmholtz is the one generally accepted, but the total amount of work performed by gravitation in the condensation of the sun from a nebulous mass to its present size could only have afforded twenty million

years' heat at the present rate of radiation. On the assumption that the sun's density increases towards the center, a few additional million years' heat might be obtained. But on every conceivable supposition gravitation could not have afforded more than twenty or thirty million years' heat.

Professor Hæckel may make any assumption he chooses about the age of the sun, but he must not do so in regard to the age of the sun's heat. One who believes it *inconceivable* that matter can either be created or annihilated may be allowed to maintain that the sun existed from all eternity, but he cannot be permitted to assume that our luminary has been losing heat from all eternity.

If 20,000,000 or 30,000,000 years do not suffice for the evolution theory, then either that or the gravitation theory of the origin of the sun's heat will have to be abandoned.

In a former paper (Quarterly Journal of Science for July, 1877) I have proved from geological evidence that the antiquity of our habitable globe must be at least three times greater than it could possibly be had the sun derived its heat simply from the condensation of its mass. This proves that the gravitation theory of the origin of the sun's heat is as irreconcilable with geological facts as it is, according to Hæckel, with those of evolution, and that there must have been some other source, in addition, at least, to gravity, from which the sun derived its store of energy.

That other source is not so inconceivable as has been assumed, for it is quite conceivable that the nebulous mass from which the sun was formed by condensation might have been possessed of an original store of heat previous to condensation. And this excessive temperature may be the reason why the mass existed in a nebulous or rarefied condition. Now if the mass were originally in a heated condition then in condensing it would have to part not merely with the heat of condensation, but also with the heat it originally possessed.

The question then arises—By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained? The dynamical theory of heat affords, as was shown several years ago (Phil. Mag. for May, 1868), an easy answer to this question. The answer is that the energy in the form of heat possessed by the mass may have been derived from *motion in space*. Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would, by their concussion, generate in a *single moment* 50,000,000 years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess $4{,}149 \times 10^{28}$ foot-pounds of kinetic energy, and this, converted into heat by the stoppage of their motion, would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

There is nothing very extraordinary in the velocity which we have found would be required to generate the 50,000,000 years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching

so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within eighty-six miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision. But more than one-half of this velocity, or 274 miles per second, would be derived from their mutual attraction as they approached each other. We have consequently to assume an original or projected velocity of only 202 miles per second. If the original velocity was 678 per second, this, with the 274 derived from gravity, would generate an amount of heat which would suffice for 200,000,000 years. And if we assume the original velocity to have been 1,700 miles per second, an amount of heat would be generated in a single moment which would suffice for no less than 800,000,000 years.

It will be asked, where did the two bodies get their motion? It may as well, however, be asked, where did they get their existence? It is just as easy to conceive that they always existed in motion as that they always existed at rest. In fact, this is the only way in which energy could remain in a body without dissipation into space. Under other forms a certain amount of it is constantly being transformed into heat which never can be retransformed back again, but is dissipated into space as radiant heat. But a body moving in void stellar space will retain its energy in the form of motion undiminished and untransformed for ever, unless a collision takes place.

The theory that the sun's heat was originally derived from motion in space is, therefore, for this reason, also more in harmony with evolution than the gravitation theory, because it explains how the enormous amount of energy which is being dissipated into stellar space may have existed in the matter composing the sun untransformed during bygone ages. Or in fact for as far back as the matter itself existed.

In conclusion there are only two sources conceivable from which the sun could have derived his heat. The one is *gravitation*, the other *motion in space*. The former could have afforded only about 20,000,000 or 30,000,000 years' heat, but there is in reality no absolute limit to the amount which may have been derived from the latter source, for the amount generated would depend on the velocity of motion. And when we take into consideration the magnitude of the stellar universe, the difference between a motion of 202 miles per second, and one of 1,700 miles to a great extent disappears, and the one velocity becomes about as probable as the other.

It may be urged as an objection to the theory that we have no experience of bodies moving in space with such enormous velocities as the above. This objection, for the following reason, is of no weight.

No body moving with a velocity exceeding 400 miles per second could remain a member of our solar system; and beyond our system there is nothing visible but the stars and nebulæ. These stars, however, are suns like our own, and visible because, like the

sun, they have lost their motion—the lost motion being the origin of their light and heat. Bodies moving in stellar space with these enormous velocities can have neither light nor heat, and, of course, must be invisible to us. They must first lose their motion before the kinetic energy in the form of motion can be transformed into light and heat, so as to constitute visible suns.—*Nature*, Jan. 10, 1878.

2. *Photographs at the Cordoba Observatory.*—Professor B. A. Gould, Director of the Observatory, in an Address delivered on the occasion of receiving from the Governor of the Province of Cordoba the premiums awarded at the Philadelphia Centennial Exhibition to the Observatory for Lunar and Stellar Photographs, makes the following statements respecting the photographic work of the Observatory. "All the photographs have been made upon plates of glass twelve centimeters long by nine broad, the images of the moon being 3·52 centimeters in diameter before enlargement. The photographs of the moon at the full, and in the last quarter, may, I think, be favorably compared with any I have seen of these phases; and the enlargements, forty-eight centimeters in diameter, being printed in carbon, can never fade. Mr. Rutherfurd's photograph of the first quarter is more sharply defined than ours, and will probably long continue to claim the palm. * * *

"Much of the credit of the stellar photographs is due to the pure air of Cordoba, which is incredibly transparent on those not very numerous occasions when the sky is truly clear. The impressions on glass which I exhibited were of six different clusters; the plate of the cluster *X Carinae*, containing two images each of 185 stars, and that of *Eta Argus*, or *Eta Carinae*, as we now call it, containing 180. I think there can be no reasonable doubt that many of the stars there depicted are quite as faint as the ninth magnitude; and I have the satisfaction of adding that upon other photographs, obtained since then, there are images of stars much fainter still, but how much, I am as yet unprepared to state. Since it is important that all the images should be perfectly round, without the slightest perceptible elongation, and since each of the two exposures should sometimes be for as much as ten minutes, during which period the motion of the telescope must not deviate appreciably from that of the star in its diurnal motion, the prepared collodion thus requiring its full sensitiveness for something like twenty-two minutes in the excessively dry atmosphere of Cordoba, it will be seen that the problem has not been without its difficulties.

"We have already secured measurable photographic impressions of not less than eighty-four celestial objects, of which nineteen are double stars, and the remainder clusters; so that, even should the undertaking remain paralyzed in its present state, the results may be regarded as richly worth the labor, money, and perhaps even the other sacrifices, that they have cost. I may add that the planets *Jupiter*, *Mars* and *Saturn* have likewise been photographed with sufficient distinctness to show clearly the details of light and color on the surfaces of the two former, and the

existence of the ring in the latter; but these images have not been sufficiently sharp to permit of successful photographic enlargement."

3. *Moon's Zodiacal Light;* note by E. S. HOLDEN. From a letter to the Editors, dated Naval Observatory, Washington, D. C., February 4, 1878.—In your Journal for February, 1878, p. 88, is a note by M. Trouvelot, on "the Moon's Zodiacal Light," in which he describes a conical luminous appendage about $4\frac{1}{2}°$ long, extended on both sides of the moon, which was seen by him April 3, 1874.

In this connection an observation of a similar phenomenon by Messier, in the Mémoires de l'Académie Royale des Sciences, 1771, p. 434, is noteworthy. In this memoir, Messier gives a rough wood-cut of its appearance, from which its length on each side of the moon is shown to be about $2\frac{1}{2}°$.

The condition of the sky, as described by Trouvelot and Messier, appears to have been the same.

In the Comptes Rendus, July 2, 1877, p. 44, M. Hugo describes a similar phenomenon which he saw above the lunar disc, about 4° in length, and in this case, also, the sky appears to have been similarly affected. These are the only cases known to me.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Address of the President of the Royal Society*, at the Anniversary Meeting, November 30, 1877.—The first part of the address relates to the affairs and operations of the society during the past year. After an account of the present constitution and condition of the Meterological Council, and a reference to an anticipated re-constitution of "a National Meterological Office under the undivided control of a man of high scientific attainments," also with some remarks on the report of the Naturalists of the Venus Transit expedition to Rodriguez and Kerguelen Island,—"The President passed on to a review of the scientific results of the Polar expedition, which he said in his judgment, especially the biological results, appeared to have quite come up to our expectations. Considering that but one season was available for collecting and observing (and we all know how short that is in the arctic regions), the results are indeed most creditable to the gentlemen who contributed them. Geology has proved by far the most prolific field of research. Perhaps Botany comes next. The researches in this department and the insects, which have been worked up by Mr. M'Lachlan, prove that between 80° and 83° N., in Grinnell's Land, the conditions for the existence of these organisms are far more favorable than are those of lands a long way to the southward. The flora of the series of channels between 80° and 83° N., the shores of which have been botanized by the officers of the Polar Expedition, has yielded upward of seventy flowering plants and ferns, which is a much greater number than has been obtained from a similar area among the polar islands to the southwestward, and is unexpectedly large. All are from a

much higher latitude than has elsewhere been explored botanically, except the islets off the extreme north of Spitzbergen. The species are, with two single exceptions, all Greenlandic. Spitzbergen, altogether to the south of these positions, contains under one hundred flowering plants and ferns, though its west coast is washed by the Gulf Stream, and its shores have been diligently explored by many trained collectors. Its north coast has yielded fewer plants, and no less than fifteen of the plants collected by the Expedition have not been found anywhere in Spitzbergen. Contrasted with Melville Island, in lat. 75° N., and Port Kennedy, in 72° N., the contrast is even more striking, these well-hunted spots, both so much farther south, yielding only sixty-seven and fifty-two species respectively. This extension of the Greenland flora to so very high a latitude can only be accounted for by the influence of warm currents of air, or by the air being warmed by oceanic currents during some period of the summer; and we look with great interest to the meteorological observations made during the voyage, which are being discussed by Sir George Nares, who hopes to have them completed in a couple of months. The observations on the temperature of sea-water will, he expects, give new information; and great interest is attached to the study of certain warm gales and warm currents that were experienced in lat. 82° and 83° N. May not these phenomena of vegetation and temperature indicate the existence of large tracts of land clothed with vegetation in the interior of Greenland, far within the mountain ranges of its ice-clad coast, and protected by these from the heavier snow-falls, and hence from the accumulation of glacial ice that surrounds it on all sides? Professor Heer, of Zurich, has examined the fossil plants, the most important of which are those he states to be of Miocene age. There are twenty-five identifiable species, of which all but one have been found also in Spitzbergen. This tracing the Miocene flora so far to the northward was one of the principal scientific objects to be accomplished by the Polar Expedition; and the fact that its character continues to be neither Polar nor Arctic, but temperate, supports the hypothesis that during the era in question a vegetation analogous to that now inhabiting the temperate latitudes entirely capped the North Polar area of the globe. Mr. Etheridge has worked at the very valuable collection of Paleozoic fossils, procured by Captain Fielden, and these, with the Miocene and Post-pliocene fossils, have thrown more light on the former conditions of the circumpolar regions than perhaps all those of previous expeditions." * * *

A reference is made to the Challenger Expedition and its scientific collections. As to the study of the latter, it is remarked that "Sir W. Thomson has, with the approval of the Council and the Government, chosen for his collaborators the ablest living specialists, and this irrespective of their nationality. It is creditable to our country that, with but few exceptions, it has supplied entirely competent and willing workers in most of the departments, while their association with such naturalists as Agassiz and Hæckel

cannot fail to be gratifying to themselves and assuring to the public." * * *

The remainder of the President's address was occupied with reference to some of the American institutions he visited during his recent tour, followed by some remarks on the flora. These he gave in more of a narrative form than characterized the formal notices of the earlier part of the address. Harvard College Observatory is particularly mentioned, and a gratifying account of its work under the new Director is given. The very limited time at the President's command prevented him from visiting other American observatories or similar institutions. The same was the case as to the natural history museums generally. In fact his whole time was given to field-work in the region west of the Great Plains. The commendation of the U. S. scientific surveys would doubtless have been extended to other examples, except that the one here so highly and justly praised was the only one which the President had had the opportunity to become personally acquainted with.

"The next subject referred to was the United States scientific surveys, of which none, the President said, has effected so much for science as that directed by Dr. Hayden. Its publications, distributed with great liberality, are in every scientific library. After a sketch of the history of the surveys, the President continued,—The most important scientific results hitherto derived from the labors of Dr. Hayden and his parties are unquestionably the geological: such as the delineation of the boundaries of the Cretaceous and Tertiary seas and lakes that occupied more than one basin of the mountains of Central North America, the ancient shores of which have yielded a marvelous accumulation of fossil vertebrates. Over an area of many hundred thousand square miles in North America there have been found, within the last very few years, beds of great extent and thickness, of all ages from the Trias onward, containing the well-preserved remains of so great a multitude of flying, creeping, and walking things, referable to so many orders of plants and animals, and often of such gigantic proportions, that the paleontologists of the States, with museums vastly larger than our own, are at a loss for space to exhibit them. So common indeed are some of these remains, and so beautifully preserved, that numbers of them, especially insects, plants and fishes, are exposed for sale with confectionery and fruit at the stalls of the railway stations, from the eastern base of the Rocky Mountains all the way to California, and are eagerly purchased by travelers. An examination of some of these fossils has yielded the important fact that in North America there is no recognized break between the Cretaceous and Tertiary beds, a vast lignitic series occupying a position that has long been disputed, and the evidence of whose fossils is very conflicting. In respect of this bed Dr. Hayden, who has traced it over many hundred miles, observes that the character of its paleontological, as well as of its strictly geological contents is such,

that it is not a matter of importance whether the entire group be placed in the lower Tertiary or upper Cretaceous; and it is most probable that the testimony of paleontologists will always be as conflicting as it is at present. Professor Marsh, one of the highest authorities in America, has not found that even fossil animals afford a satisfactory solution of the difficulty. 'Invertebrate remains,' he says, 'throw little light on the question;' and he is obliged to assume that 'the line, if line there be, must be drawn where the dinosaurs and other Mesozoic vertebrates disappear, and are replaced by the mammals, henceforth the dominant type.' In reference to the disputed horizons of the Cretaceous and Eocene beds, he concludes that plants afford unsatisfactory measures of geological time—an opinion which I had long ago expressed. We are also agreed as to a chief cause of this being the comparatively low organization of plants, which are hence less subject to the influences of environing conditions; to which cause might be added, almost as a corollary, the feeble conflict among the members of the vegetable kingdom as compared with the animal, their stationary habits, and that consequent duration of similar, if not identical, forms, through long geological ages, which has always appeared to me to be one of the most signal characteristics of the early condition of the higher plants as compared with the higher animals. Other, and perhaps even more sufficient, reasons for plants being so little satisfactory is that their reproductive organs, those upon which the classification is principally based, are rarely preserved, and seldom in connection with the vegetative organs, which are abundantly preserved; and that, with regard to these, the vegetative organs, their prevalent and best preserved characters, outlines and venation, vary in individual species to a surprising degree, and, being repeated in groups otherwise in no way related, become too often fallacious guides. Another result, foretold in respect of other organisms, but ably worked out by Professor Marsh in respect of the vertebrates, is that all the Tertiary beds of North America—Eocene, Miocene and Pliocene—are of older data than the corresponding ones in Europe. This, though apparently supported by his conclusions that the main migrations of animals was from the American to the Asiatic continent (which he deduces from the American, as compared with the European, life-histories of the Edentata, Marsupialia, Ungulata, Rodentia, Carnivora and even Primates, is a very bold generalization. Without presuming to question the abundance and teachings of the American data, I cannot but think that his conclusion as regards migration is, in the present state of paleontology, premature, especially under our almost absolute ignorance of the vertebrate fossils of the continents of Asia and Africa." * * *

A reference is made to "a few of the magnificent collections of vegetable remains, Cretaceous and others, that have been studied and published by Mr. Leo Lesquereux in various reports of the United States Geological Survey, and in separate works issued under its auspices;" and a series of critical remarks upon vegetable paleontology and its peculiar difficulties and liabilities follow. We

have not room for these, nor for the concluding portion of the address, which gives in brief summary the impressions in regard to the flora of the country,—the result of personal observation during a rather rapid but favorably-circumstanced reconnoissance from the Atlantic to the Pacific. We add merely the final paragraph of the address.

"I must not close my notice of some of the labors of our scientific brethren in the United States without expressing my admiration of the spirit and the manner in which the Government and people have coöperated in making known the physical and biological features of their country, and my conviction that the results they have given to the world are, whether for magnitude or importance, greater of their kind than have been accomplished within the same time by any people or Government in the older continents. How great would now be our knowledge of the climate and natural features of India had its trigonometrical or revenue surveys been carried out in the same catholic spirit? And what scientific literature can England and its colonies show to compare with that of the United States surveys?"

These extracts were taken from the report in the Times newspaper, of November 30, in advance of the official copy in the Royal Society's Proceedings.

2. *Observations on Hermetically-sealed Flasks opened on the Alps.* In a letter to Professor HUXLEY, from Professor TYNDALL, dated Alp Lusgen, September 18th, 1877.—Though the question of "Spontaneous Generation" is, I believe, practically set at rest for the scientific world, you may possibly deem the following facts of sufficient interest to be communicated to the Royal Society.

I brought with me this year to the Alps sixty hermetically-sealed flasks, containing infusions of beef, mutton, turnip and cucumber, which had been boiled for five minutes in London and sealed during ebullition. They were packed in sawdust, and when opened at Bel-alp the drawn-out and sealed ends of six of them were found broken off. These six flasks were filled with organisms, while the remaining ones were pellucid and free from life.

Two or three of them were subsequently broken by accident, but for six weeks fifty of the flasks remained perfectly clear.

At the end of this time I took twenty-three of them into a shed containing some fresh hay, and there snipped off their sealed ends with a pair of pliers. The air of the hay-loft entered to fill the vacuum produced by the boiling in London. Twenty-seven other flasks were taken immediately afterward to the edge of a declivity, which might almost be called a precipice, with a fall of about a thousand feet. A gentle breeze was blowing from the mountains, which were partly snow-covered and partly of bare rock, toward the precipice. Taking care to cleanse my pliers in the flame of a spirit-lamp, and to keep my body to leeward of the flasks, I snipped off their sealed ends.

The two groups of flasks were then placed in our own little kitchen, where the temperature varied from about 65° to 90° Fahrenheit.

Result:—Twenty-one of the twenty-three flasks opened on the hay-loft are filled with organisms; two of them remain clear.

All the flasks opened on the edge of the precipice remain as clear as distilled water. Not one of them has given way.

This is a striking confirmation of the experiments of Pasteur upon the Mer de Glace.—*Proc. Roy. Soc.*, No. 184.

3. *Researches on the Effect of Light upon Bacteria and other Organisms;* by ARTHUR DOWNES, M.D., and THOS. P. BLUNT, M.A. —The authors, after giving the details of their careful and extended observations announce the following conclusions.—The deductions which we draw from these simple experiments may be summed up as follows:—

(1.) Light is inimical to the development of *Bacteria* and the microscopic fungi associated with putrefaction and decay, its action on the latter organisms being apparently less rapid than upon the former.

(2.) Under favorable conditions it wholly prevents that development, but under less favorable it may only retard.

(3.) The preservative quality of light, as might be expected, is most powerful in the direct solar ray, but can be demonstrated to exist in ordinary diffused daylight.

(4.) So far as our investigation has gone it would appear that it is chiefly, but perhaps not entirely, associated with the actinic rays of the spectrum.

(5.) The fitness of a cultivation-liquid to act as a nidus is not impaired by insolation.

(6.) The germs originally present in such a liquid may be wholly destroyed, and a putrescible fluid perfectly preserved by the unaided action of light.

Although there are many vital phenomena, both of plant-life and of animal, whether in health or disease, to the elucidation of which may be applied this quality of light (now demonstrated, so far as we are aware, for the first time), we have endeavored in this paper to confine ourselves to the plain facts of our observations, and have studiously avoided speculation and theory. We cannot, however, refrain from offering one comment on the striking antagonism between these facts and many views that have hitherto prevailed on the relation of light to life. This relation has been principally investigated as regards the chlorophyl-cell; but chlorophyl may be regarded as simply an organ of nutrition adapted to special circumstances, and differing essentially in its vital phenomena from the true cellular tissue of the plant and its protoplasmic contents.

It appears to us that the organisms which have been the subject of our research may be regarded simply as individual "cells" or minute protoplasmic masses specially fitted by their transparency and tenuity for the demonstration of physical and other influences. May we not expect that laws similar to those which here manifest themselves may be in operation throughout the vegetable, and perhaps also the animal, kingdom wherever light has direct access

to protoplasm? On the one hand we have chlorophyl, owing its very existence to light, and whose functions are deoxidizing; on the other the white protoplasm, or germinal matter, oxidizing in its relations, and to which, in some of its forms at least, the solar rays are not only non-essential, but even devitalizing and injurious.

This suggestion we advance provisionally and with diffidence; nor do we wish to imply that the relations of light to protoplasmic matter are by any means so simple as might be inferred from the above broad statement.

To this the following is added by the authors as the inference from other experiments described in a postscript.

This remarkable fact, then, appears to follow as a deduction, that a vacuum (or approximation to such) which of itself is a condition antagonistic to the development of *Bacteria*, nevertheless shields these organisms from the germicidal effect of light.* *Proc. Roy. Soc.*, No. 184.

4. *Expenditures for Universities in Germany.*—Some interesting details, on the contributions of the State to the universities, as well as on other points, were given in a recent number of the Academy by Professor Ray Lankester:—

"The sum expended by the North German States on the twenty universities belonging to them is annually more than £500,000. The Imperial Government has expended upon the new University of Strassburg alone £70,000 in one year. The University of Leipzig alone receives annually from the Saxon Government over £50,000. There are eight universities in North Germany which are little, if at all, less costly, and there are eleven of smaller size which receive each from £8,000 to £20,000 annually.

"In North Germany there is one university to every two million inhabitants; in Austria there is one to every five million; in Switzerland one for each million; in England one to every seven millions. In the twenty North German universities there are 1,250 professors.† In the British Islands we ought to have sixteen universities and 1,000 professorships in order to come up to the same level in this respect as North Germany. The stipend (apart from fees) of a professor in a German university ranges from £100 to £600 a year. As a rule, at the age of five-and-thirty, a man in this career may (in Germany) count on an assured income of £400 a year (with retiring pension). The expenditure on attendants, libraries, laboratories and officials may be calculated as being (in a well-conducted university) more than equal in amount to the total of the professor's stipends. Taking the *average* German professorial stipend at only £200 a year, we find that £250,000 must be spent annually on this item alone in the North German States.

"In order to equip and carry on sixteen universities in this country which should bear comparison with the German universi-

* We wish, however, to make it clear that we by no means insist on this explanation; the facts, indeed, admit of other explanations.

† *i. e.*, We presume professors strictly so-called, exclusive of "privat-docenten."

ties, we require not less than an immediate expenditure of £1,000,000 sterling in building and apparatus, and an annual expenditure of from £500,000 to £800,000."

When we add to the Government subsidy the income of the universities from other sources, the sum is enormously increased. The half-million, moreover, does not include the occasional grants of the Government for special purposes. Some idea of the magnificence of these was shown in our recent "University Intelligence," where it was stated that in the budget submitted to the present Prussian House of Deputies are the following items:—Erection of the German Industrial Museum, 998,000 mk.; erection of a Polytechnic in Berlin, 8,393,370 mk.; erection of an Ethnological Museum in Berlin, 1,800,000 mk.; and for the Berlin University, erection of an Herbarium, 422,000 mk.; of a Clinic, 1,955,000 mk.; of a new building for a second Chemical Laboratory, as well as of a Technical and Pharmaceutical Institute, 967,000 mk.

Of the nature and extent of the scientific teaching in German universities some idea may be formed from the subjects represented by the teaching staff at Berlin, which may fairly be taken as a type of the whole. In Berlin then we find that there are (excluding the privat-docenten) five professors of mathematics, two of astronomy, seven of chemistry, five of physics, three of geology, four of botany, two of zoology, one of meteorology, two of geography, one of anthropology and one of agriculture—physiology and comparative anatomy being well represented in the medical faculty, and we might well have included among teachers of science those who devote themselves to the scientific investigation of languages. But a mere statement of the number of teachers gives no adequate idea of the means at the command of a German University for training its students in science. The number of teachers in each subject secures that its various departments will be thoroughly worked out, and gives a student a chance of following out any specialty he may take up; this is made still further possible by the number and variety of institutions, museums, laboratories, collections, &c., attached to each university, not to speak of its large and comprehensive library. In connection with Berlin alone there are twenty-three scientific "Anstalten," as they are called, for practical investigation in connection with the various faculties. Had we taken the numerous "Realschulen" and the high and polytechnic schools into account, where an education can be obtained quite equal to that obtainable at most of our universities and colleges, it would have been seen that higher education in Germany leaves little to be desired.—*Nature*, Dec. 6, p. 104.

5. *Earthquake of Nov.* 15, 1877.—In addition to the notice on page 21 of this volume, the United States Weather Review for November contains reports from thirty-five stations, from which it appears that the shock was felt throughout the whole of Iowa and Nebraska, extending also into Kansas and Missouri on the South, and into Dakota and Minnesota on the North. In Omaha

three, and in North Platte, Lincoln and West Point, Neb., two shocks were felt. In North Platte and Columbus, Neb., and in Sioux City, Iowa, walls of buildings were cracked. The times given are still very discordant, varying all the way from 10.40 A. M., to 12.20 P. M., Omaha time. The time at Omaha, 11.45 A. M., is probably not far from correct. The December number of Peterman's Mittheilungen, contains a long article on the Iquique earthquake of May 9, 1877, giving valuable data in regard to shock and ocean wave. C. G. R.

6. *Memoirs of the Geological Survey of India, Palæontologia Indica.* Section II, No. 2, contains Jurassic (Liassic) flora of the Rajmahel Group in the Rajmahel Hills, by OTTOKAR FEISTMANTEL, M.D. Calcutta, 1877. 4to, with 13 plates, including descriptions of several species of *Pterophyllum* and other Cycadeous plants, besides Ferns of the genera *Cyclopteris*, *Pecopteris*, *Alethopteris*, *Asplenites*, *Thinnfeldia*, etc.

7. *Volunteer Weather Service.*—A Volunteer Weather Service has been started in Missouri. It is undertaken under the auspices of Washington University, St. Louis, which will be recognized as the "central station." The circular issued is signed by William S. Eliot, President of the University, and Francis E. Nipher, Professor of Physics.

8. *Beiblätter zu den Annalen der Physik und Chemie;* Publishing House of Johann Ambrosius Barth, Leipzig.—The Annalen der Physik und Chemie, which was edited for upwards of fifty years by Professor Poggendorff, is well known by physicists as occupying the first place among German Physical Journals. The *Beiblätter* form a supplementary though independent series, containing abstracts of all physical articles published in the proceedings of societies, in scientific journals, or in other forms, not only in Germany, but also in all other civilized countries. The volume for the year 1877, lately completed, is thus a complete Repertorium for all the recent advances in physical science. The value of such a summary to workers in this branch of science, to whom much of the literature would be otherwise inaccessible, can hardly be overestimated.

OBITUARY.

M. ANTOINE CÉSAR BECQUEREL died on the 19th of January, at the advanced age of nearly ninety years. He was born at Châtillon-sur-Loing (Loiret) on the 7th of March, 1788. M. Becquerel was educated in the Polytechnic School, which he left as an engineer officer in 1808. He served in Spain and took part in several sieges under the orders of Marshal Suchet. In 1814 M. Becquerel was named Inspector of the Polytechnic School, and he quitted the army in 1815.

M. Becquerel was elected a Member of the Académie des Sciences in April, 1829, and a Corresponding Member of the Royal Society of London in 1837, the Copley medal being awarded to him for his researches in electricity. He was Professor of Physics in the Muséum d'Histoire Naturelle. A large number of memoirs

on different branches of electricity, to which M. Becquerel devoted his especial attention, will be found in the Comptes Rendus of the Academy of Sciences. We may more particularly name Mémoires sur les Caractères Optiques des Minéraux (1838), Sur les Propriétes Électro-Chimiques des Corps Simples et leurs Applications aux Arts (1841), and Mémoires sur la Reproduction Artificielle des Composés Minéraux, à l'aide de Courants Électriques très faibles (1852). His researches on animal heat, and other applications of physics to physiology, on which subjects memoirs will be found in the Comptes Rendus, were of a high class.

M. Becquerel was a voluminous writer on science, the most important of his works being, Traité de l'Électricité et du Magnétisme (1834–1840, in seven vols.), Traité d'Électro-Chimie, and his Traité de Physique appliquée à la Chimie et aux Sciences Naturelles. Beyond these he published, in connection with his son, M. Edmund Becquerel, several works on meteorology, on agricultural chemistry, on the influences of forests on climate, and on the several divisions of electrical science, to which the father and son had devoted the largest portion of their lives.—*Athenæum*, Jan. 26.

M. Regnault.—M. Henri Victor Regnault died at Paris, almost simultaneously with M. Becquerel, on the 21st of January.

M. Regnault was born on the 21st of July, 1810, at Aix-la-Chapelle He was a student of the Polytechnic School, and shortly after leaving that school he became Ingénieur en Chef des Mines. In 1840 he became Professor of Physics in the College of France and of Chemistry in the Polytechnic School. In the same year he was elected a Member of the Académie des Sciences, and in 1854 he became Director of the Manufactory of Porcelain at Sèvres. In 1852 M. Regnault was elected a Foreign Member of our Royal Society, and at different times the Copley and the Rumford Medals were presented to him. M. Regnault was also a Corresponding Member of the Academies of Berlin and St. Petersburg. In the Annales de Chimie et de Physique and in the Comptes Rendus des Séances de l'Académie will be found numerous memoirs by this eminent chemist. One of the most important works published by M. Regnault appeared in the twenty-first volume of Mémoires de l'Académie des Sciences, under the title of Relation des Expériences entreprises par Ordre de M. le Ministre des Travaux Publics, et sur la Proposition de la Commission Centrale des Machines à Vapeur.' These researches remain a standard authority upon all questions relating to the theory and practice of the use of steam as a motive power.

M. Regnault was the father of the celebrated painter who fell, fighting for his country, at the siege of Paris.

M. Regnault published a Cours Élémentaire de Chimie, in four volumes, Première Notions de Chimie, and a Traité de Physique. The Cours Élémentaire has been translated into several European languages, and the other works of M. Regnault are highly appreciated in this country as in France.—*Ibid.*

APPENDIX.

ART. XXXIII.—*Notice of New Dinosaurian Reptiles;* by Professor O. C. MARSH.

IN addition to the Jurassic reptiles already described by the writer,* several others of interest are now represented in the Yale Museum. Among these are a number of Dinosaurs of gigantic size, and others of diminutive proportions. Nearly all are from the Atlantosaurus beds of the Rocky Mountains. Most of the larger specimens belong to the *Atlantosauridæ*, a group marked by some interesting characters not before observed in Dinosaurs. The more important of these characters, so far as at present known, are, the pneumatic cavities in the vertebræ; the sacrum with only three or four vertebræ, and a corresponding short ilium; the large fore limbs; and the presence of five well developed digits in both the manus and pes. The latter was ungulate, and essentially plantigrade. The carpal and tarsal bones are not coössified with the long bones, and the third trochanter of the femur is rudimentary or wanting. The known genera are *Atlantosaurus* (*Titanosaurus*), *Apatosaurus*, and *Morosaurus* described below.

Atlantosaurus immanis, sp. nov.

The present species was vastly larger than any land animal either recent or fossil, hitherto described. It is indicated by various well preserved remains, of which the most characteristic is the femur. This bone has no true head, and no distinct third trochanter. The proximal end and upper half of the shaft are compressed transversely. The inner condyle of the distal end is proportionally large, and on the outer one, the fibular ridge is well marked. This femur is *over eight feet* (98 inches, or 2,500^{mm}) in length. The transverse diameter of the proximal end is 25 inches (635^{mm}), and the antero-posterior diameter of the inner condyle of the distal end is 18 inches (475^{mm}). A comparison of this bone with the femur of a Crocodile (*C. Americanus*), would indicate for the fossil species, supposing the two reptiles to have the same proportions, a length of about one hundred and fifteen feet!

* This Journal, xiv, pp. 87, 254, 513, 514.

The other bones found near the femur are proportionally gigantic, one caudal vertebra having a transverse diameter of over 16 inches (420·mm). That this reptile when alive was near one hundred feet in length, is probable, although it may have been much less.

The only remains of this monster at present known are in the Yale College Museum. They are from the Upper Jurassic of Colorado.

Morosaurus impar, gen. et sp. nov.

This genus is allied to *Apatosaurus* and *Atlantosaurus*, but may be distinguished from them by the sacrum, as well as by other characters. The former has but three sacral vertebræ, while the present genus has four. The transverse processes are vertical plates, except at their expanded ends, which extend below the inferior surface of the centra. The latter are also more fully ossified than in *Atlantosaurus*. The first sacral vertebra has its articular face somewhat convex, while the articulation of the last sacral vertebra is concave.

The present species is represented by various remains, the sacrum being most characteristic. Its principal dimensions are as follows:

Length of sacrum	535·mm
Transverse diameter of anterior articular face	215·
Transverse diameter of posterior articular face	190·
Expanse of transverse processes of second vertebra	395·

This sacrum indicates a reptile at least twenty-five feet in length. It was found with other remains in the Atlantosaurus beds by Mr. S. W. Williston, of Yale College Museum, to whom science is indebted for many important discoveries in the Rocky Mountain region.

Allosaurus lucaris, sp. nov.

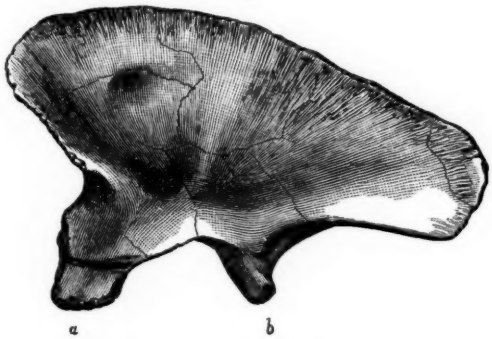
The peculiar genus named by the writer *Allosaurus* proves to be very different from the Dinosaurs found with it, and to represent a distinct family, *Allosauridæ*. A second species, apparently of this genus, is indicated by some characteristic remains among which is an anterior dorsal vertebra. This has the anterior articulation of the centrum somewhat convex, and the posterior face concave. The sides of the centrum are so deeply excavated that only a narrow keel is left below, and there are large cavities in the interior. The length of this centrum is 69·mm; the vertical diameter of the anterior face, 81·mm; and the width of this face, 83·mm. The articulation for the rib is at the anterior border, just below the suture of the neural arch.

This specimen is from the Upper Jurassic of the Rocky Mountains, and belonged to a reptile eighteen or twenty feet in length.

Creosaurus atrox, gen. et sp. nov.

This genus is nearly allied to *Dryptosaurus* (*Lælaps*), and was the carnivorous enemy of the huge *Atlantosauridæ*. It is indicated by various remains in excellent preservation, among them the ilium represented below. The teeth referred to the present species have the crowns more or less trihedral, and the cutting edges crenulated. The metapodial bones preserved are elongated, and the terminal phalanges supported sharp claws. The vertebræ known are biconcave, and the terminal caudals are much elongated.

1.



Left ilium of *Creosaurus atrox* Marsh. Outside view.

2.

Inferior view. Both one-tenth natural size.

The following measurements indicate the size of this reptile:

Antero-posterior diameter of left ilium	700^{mm}
Vertical diameter	425·
Length of metatarsal	277·
Transverse diameter of proximal end	72·
Transverse diameter of distal end	79·
Length of distal caudal vertebra	78·
Transverse diameter of proximal end	33·
Transverse diameter of distal end	31·

This animal was about twenty feet in length. The remains at present known are from the same horizon as those above described, and were collected by Mr. S. W. Williston.

Laosaurus celer, gen. et sp. nov.

The present genus is indicated by various remains of small Dinosaurs, of two or more species. The long bones are not hollow like those of *Nanosaurus*, but their walls are thick, and the cavities small. The vertebræ preserved are biconcave, and the neural arches loosely united to the centra. The dorsal and anterior caudals are more elongated than in most Dinosaurs. The phalanges are so avian in character, that they would readily be taken for those of birds. The anterior limbs were much smaller than the posterior.

The following are some of the dimensions of the present species:

Length of median caudal vertebra	24·mm
Vertical diameter of anterior articulation	17·
Transverse diameter	16·
Greatest diameter of proximal end of ulna	19·5
Length of proximal phalanx of pes	29·
Length of second phalanx of pes	21·
Length of third phalanx	16.

The remains at present known indicate an animal about as large as a fox. They are from the same horizon as the species described above.

Laosaurus gracilis, sp. nov.

A second species, much smaller than the above, is represented by well preserved remains of various parts of the skeleton. Its size is indicated by the following measurements:

Length of lumbar vertebra	16·mm
Transverse diameter of anterior face	18·
Transverse diameter of posterior face	17·
Length of median caudal vertebra	16·
Transverse diameter anterior face	12·
Greatest diameter of proximal end of ulna	17·

The present species is from the same locality and horizon as the one above described.

This reptile is the smallest known Dinosaur, with the exception of the diminutive species of *Nanosaurus* (*N. agilis* and *N. victor*). The latter genus possesses some very peculiar characters, and represents a distinct family, *Nanosauridæ*.

Yale College, New Haven, February, 1878.